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The Gift
of
John W. Webster, M.D.
one of the Editors

45-55
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THE BOSTON JOURNAL OF Philosophy and the Arts,

INTENDED TO EXHIBIT A VIEW OF THE

PROGRESS OF DISCOVERY

IN

NATURAL PHILOSOPHY, MECHANICS, CHEMISTRY, GEOLOGY AND MINERALOGY,
NATURAL HISTORY, COMPARATIVE ANATOMY AND PHYSIOLOGY,
GEOGRAPHY, STATISTICS,
AND THE FINE AND USEFUL ARTS.

CONDUCTED BY
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ADVERTISEMENT.

The present number completes the third volume of this journal, and with it the editors take their leave of the public. From the commencement of their work, the editors have never expected an extensive subscription. The pursuits of this busy and thriving community do not lead it to a very lively interest in the subjects to which this publication has been devoted, and although the subscription has been sufficient to defray the expense of printing, the labour of conducting the work has been without reward.

Under these circumstances, the editors cannot any longer devote the time necessary to make selections from foreign works, and to furnish such original matter as shall make their journal satisfactory to themselves or worthy of their subscribers. Far from intending to express any thing like disapprobation or complaint that a larger portion of the public have not a taste for the kind of reading which has been presented in this journal, the editors have made the preceding statement merely to show their patrons, openly, the cause of the discontinuance of their work. The editors feel themselves warranted in attributing their want of patronage to the peculiarity of the public taste, in regard to works of this kind, generally, rather than to the individual character of the papers which they have published, from the fact, that the selected articles, which comprise the greater part of this journal, were written by men most celebrated for genius and attainments in Europe, and on subjects which have occupied a great share of the attention of the scientific world during the progress of this publication.

Boston, December, 1826.

THE

BOSTON JOURNAL

OF

PHILOSOPHY AND THE ARTS.

Art. I.—*Biographical Account of M. de Fourcroy.* By Thomas Thomson, M. D. F. R. S. [*Ann. Philos.*]

Literary men may be divided into three classes. Some make a great figure during their life time; but death erases their names from the annals of science, and they sink into the grave and obscurity at once. Such were Dr Mead and Sir John Hill. Some are little known during their life time, and spend their days in obscurity and penury; but when death has once closed the scene, their reputation rises untarnished by envy, and unsullied by emulation, and flows on like a mighty river, the broader, and deeper, and greater, the farther it advances. Such, in some respects, were Kepler and Scheele. Some are so unfortunate, through imprudence, or a perverse train of circumstances, neither to acquire reputation during their lives, nor after their death; while their more fortunate contemporaries, with less labour, and less merit, gather all the laurels which they had earned. It would be invidious to mention the names of any who unfortunately belong to this class; but they will readily occur to every one acquainted with the history of science. Every tyro in algebra is familiar with Cardan's rules for the solution of cubic equations, while the name of the real discoverer of these rules is scarcely known, except to mathematical antiquaries. M. de Fourcroy, the subject of this article, made so conspicuous a figure during his life time, that it would by no means surprise us, if he should finally take his place among that class of literary men, whom we characterised in the first place; not that he wanted merit; for it is not so much merit, as a regard to distributive justice, which leads to the classification. Who will be hardy enough to affirm that Churchill wanted merit as a poet? During his short and rapid literary career he appeared to wield the thunderbolts in his hand, and was an object of dread and adoration, like a kind of Divinity. But where is his reputation now? It has sunk, since his death, as much below the true level, as it rose above it during his life time; and this we believe will always be the case. Mankind will atone for the excessive adulation which they pay to a man during his lifetime, by a corresponding negligence after his death.

Antoine François de Fourcroy, Compte of the French empire, Counsellor of State, Commander of the Legion of Honour, Member of the Institute, and of most scientific societies in Europe, Professor of Chemistry at the Museum of Natural History, Professor of the Faculty of Medicine at Paris, and teacher in the Polytechnic School, was born at Paris, on the 15th of June, 1755, and was the son of Jean Michel de Fourcroy and of Jeanne Laugier.

His family had long resided in the capital, and several of his ancestors had distinguished themselves at the bar. One of them, during the reign of Charles IX. was honored with the epithet of *fori decus*.

Antoine François de Fourcroy sprung from a branch of the family that had gradually sunk into poverty. His father exercised in Paris the trade of an apothecary, in consequence of a charge which he held in the house of the Duke of Orleans. The Corporation of Apothecaries having obtained the general suppression of all such charges, M. de Fourcroy, the father, was obliged to renounce his mode of livelihood; and his son grew up in the midst of the poverty produced by the monopoly of the privileged bodies in Paris. He felt this situation the more keenly, because he possessed from nature, an extreme sensibility of temper. When he lost his mother, at the age of seven years, he attempted to throw himself into her grave. The care of an elder sister preserved him with difficulty, till he reached the age at which it was usual to be sent to the college. Here he was unlucky enough to meet with a brutal master, who conceived an aversion to him, and treated him with cruelty. The consequence was a dislike to study; and he quitted the college at the age of 14, somewhat less informed than when he went to it.

His poverty now was such, that he was under the necessity of endeavouring to support himself by commencing writing master. He had even some thoughts of going upon the stage, but was prevented by the hisses bestowed upon a friend of his, who had unadvisedly entered upon that perilous career, and was treated in consequence, without mercy by the audience. While uncertain what plan to follow, the advice of Viq. d'Azyr induced him to commence the study of medicine.

This great anatomist was an acquaintance of M. de Fourcroy, the father. Struck with the appearance of his son, and the courage with which he struggled with his bad fortune, he conceived an affection for him, and promised to direct his studies, and even to assist him during their progress. The study of medicine to a man in his situation was by no means an easy task. He was obliged to lodge in a garret, so low in the roof, that he could only stand upright in the centre of the room. Beside him lodged a water-carrier, with a family of twelve children. Fourcroy acted as physician to this numerous family; and in recompense was always supplied with abundance of water. He contrived to support himself by giving lessons to other students, by facilitating the researches of richer writers, and by some translations which he sold to a bookseller. For these he was only half paid; but the conscientious bookseller offered, thirty years afterwards, to make up the deficiency, when his creditor was become Director General of Public Instruction.

Fourcroy studied with so much zeal and ardour that he soon became well acquainted with the subject of medicine. But this was not sufficient. It was necessary to get a Doctor's degree; and all the expenses, at that time, amounted to £250 sterling. An old physician, Dr Diest, had left funds to the faculty to give a gratuitous degree and license, one every two years, to the poor student, who should best deserve them. Fourcroy was the most conspicuous student at that time in Paris. He would therefore have reaped the benefit of this benevolent institution had it not been for the unlucky situation in which he was placed. There happened to exist a quarrel between the faculty charged with the education of medical men and the granting of degrees, and a society, recently established by government for the improvement of the medical art. This dispute had been carried to a great length, and had attracted the attention of all the frivolous and idle inhabitants of Paris. Viq. d'Azyr was secretary to the society, and of course one of its most active champions, and was in consequence particularly obnoxious to the faculty of medicine at Paris. Fourcroy was unluckily the acknowledged protegée of this eminent anatomist. This was sufficient to induce the faculty of medicine to refuse him a gratuitous degree. He would have been excluded in consequence from entering upon the career of a practitioner, had not the society, enraged at this treatment, and influenced by a violent party spirit, formed a subscription, and contributed the necessary expenses.

It was no longer possible to refuse M. de Fourcroy the degree of Doctor, when he was thus enabled to pay for it. But above the simple degree of Doctor, there was a higher one, entitled Docteur Regent, which depended entirely upon the votes of the faculty. It was unanimously refused to M. de Fourcroy. This refusal put it out of his power afterwards to commence teacher in the medical school, and gave the medical faculty the melancholy satisfaction of not being able to enrol among their number the most celebrated professor in Paris. This violent and unjust conduct of the faculty of medicine made a deep impression in the mind of Fourcroy, and contributed not a little to the subsequent downfall of that powerful body.

Fourcroy being thus entitled to practise in Paris, his success depended entirely upon the reputation which he could contrive to establish. For this purpose he devoted himself to the sciences connected with medicine, as the shortest and most certain road by which he could reach his object. His first writings showed no predilection for any particular branch of science. He wrote upon chemistry, anatomy, and natural history. He published an abridgment of the History of Insects, and a Description of the Bursæ Mucosæ of the Tendons. This last piece seems to have given him the greatest celebrity; for in 1785 he was admitted, in consequence of it, into the Academy of Sciences, as an anatomist; but the reputation of Bucquet, which at that time was very high, gradually directed his particular attention to chemistry, and he retained this predilection during the rest of his life.

Bucquet was at that time professor of chemistry in the medical school of Paris, and was then greatly celebrated and followed, on account of his eloquence and the elegance of his language. Fourcroy became in the first place his pupil, and soon after his particular friend. One day, when an unforeseen disease prevented him from lecturing as usual, he entreated M. de Fourcroy to supply his place. The young philosopher at first declined, and alleged his total ignorance of the method of addressing a popular audience. But, overcome by the persuasions of Bucquet, he at last consented;

and in this his first essay, he spoke two hours without disorder or hesitation, and acquitted himself to the satisfaction of his whole audience. Bucquet soon after substituted him in his place, and it was in his laboratory and in his class room that he first made himself acquainted with chemistry. He was enabled at the death of Bucquet, in consequence of an advantageous marriage which he had made, to purchase the apparatus and cabinet of his master; and although the Faculty of Medicine would not allow him to succeed to the chair of Bucquet, they could not prevent him from succeeding to his reputation.

There was a kind of college established in the King's Garden, which was at that time under the superintendance of Buffon, and Macquer was the professor of chemistry in this institution. On the death of this chemist, in 1784, Lavoisier stood candidate for the chair. But Buffon received more than a hundred letters in favour of Fourcroy; and the voice of the public was so loud in his favour, that he was appointed to the situation, in spite of the high reputation of his antagonist, and the superior interest that might be supposed to result from his fortune and his situation.

Fourcroy continued professor at the Jardin des Plantes, during the remainder of his life, which lasted twenty-five years; and such was his eloquence, or so well was it fitted to the taste of the French nation, that his celebrity as a lecturer continued always upon the increase; so great also were the crowds, both of men and women, that flocked to hear him, that it was twice necessary to enlarge the lecture-room. I had myself an opportunity of hearing him lecture two or three times, and must acknowledge that I found it difficult to account for the celebrity which he enjoyed. His style was precisely similar to that of his books, flowing and harmonious, but very diffuse, and destitute of precision; and his manner was that of a petit maitre, mixed with a good deal of pomposity, and an affectation of profundity. There must be something, however, in such a manner, capable of attracting the generality of mankind; for I know a professor, who possesses as much of it as is consistent with the British character, and who is far inferior to Fourcroy as a man of science; who nevertheless, enjoys within his own sphere, nearly the same degree of popularity that Fourcroy did in his.

We must now notice the political career which Fourcroy ran, during the progress of the revolution. In a country where political changes were going on with so much rapidity, and where every description of men were successively had recourse to, it was not possible that a professor so much admired for his eloquence, could escape observation. Accordingly he was elected a member of the National Convention in the autumn of 1793. The National Convention, and France herself, were at that time in a state of abject slavery; and so sanguinary was the tyrant who ruled over that unhappy country, that it was almost equally dangerous for the members of the Convention to remain silent, or to take an active part in the business of that assembly. Fourcroy, notwithstanding his reputation for eloquence, and the love of eclat which appears all along to have been his domineering passion, had good sense enough to resist the temptation, and never opened his mouth in the Convention till after the death of Robespierre. This is more to be wondered at, and is a greater proof of prudence, as it is well known that he took a keen part in favour of the revolution, and that he was a determined enemy to the old order of things, from which he had suffered so severely at his entrance into life.

At this period he had influence enough to save the life of some men of merit. Among others, of Darcet, who did not know the obligation he lay under to him, till long after. At last his own life was threatened, and his influence, of course, utterly annihilated.

During this unfortunate and disgraceful period, several of the most eminent literary characters of France were destroyed; among others, Lavoisier; and Fourcroy has been accused of contributing to the death of this illustrious philosopher, his former rival, and his master in chemistry. How far such an accusation is deserving of credit, I, for my part, have no means for determining; but Cuvier, who was upon the spot, and in a situation which enabled him to investigate its truth or falsehood, acquits Fourcroy entirely of the charge, and declares that it was urged against him, merely out of envy at his subsequent elevation. "If in the rigorous researches which we have made," says Cuvier in his Eloge of Fourcroy, "we had found the smallest proof of an atrocity so horrible, no human power could have induced us to sully our mouths with his Eloge, or to have pronounced it within the walls of this temple, which ought to be no less sacred to honor than to genius."

Fourcroy began to acquire influence only after the ninth thermidor, when the nation was wearied with destruction, and when efforts were making to restore those monuments of science, and those public institutions for education, which, during the wantonness and folly of the revolution, had been overturned and destroyed. Fourcroy was particularly active in this renovation, and it was to him chiefly, that almost all the schools established in France for the education of youth, are to be ascribed. The convention had destroyed all the colleges, and universities, and academies, throughout France. The effects of this ridiculous abolition, soon became visible. The army stood in need of surgeons and physicians, and there were none educated to supply the vacant places. Three new schools were founded for educating medical men. They were nobly endowed, and still continue connected with the University of Paris. The term, Schools of Medicine, was proscribed as too aristocratical. They were distinguished by the ridiculous appellation of Schools of Health. The Polytechnic school was next instituted, as a kind of preparation for the exercise of the military profession, where young men could be instructed in mathematics and natural philosophy, to make them fit for entering the schools of the artillery, of engineers, and of the marine. The central school was another institution for which France is indebted to the efforts of Fourcroy. The idea was good, though it has been very imperfectly put in execution. It was to establish a kind of university in every department, for which the young men were to be prepared by means of a sufficient number of inferior schools scattered through the department. But these inferior schools have never been properly established or endowed; and even the central schools themselves have never been supplied with proper masters. Indeed it would have been impossible to have furnished such a number of masters at once. On that account, an institution was established at Paris, under the name of Normal School, for the express purpose of educating a sufficient number of masters to supply the different central schools.

Fourcroy, either as a member of the Convention, or of the Council of Ancients, took an active part in all these institutions, both as far as regarded the plan and the establishment, He was equally concerned in the establishment of the Institute, and of the Museum d'Histoire Naturelle. This last was endowed with the utmost liberality, and Fourcroy was one of the first professors; as he was, also, in the school of medicine, and the Polytechnic school. He was equally concerned in the restoration of the University, which constitutes the most splendid part of Buonaparte's reign, and the part which will be longest remembered with gratitude and applause.

The violent exertions which M. de Fourcroy made in the numerous situations which he filled, and the prodigious activity which he displayed, gradually undermined his constitution. He himself was sensible of his approaching death, and announced it to his friends as an event which would speedily take place. On the 16th of December, 1809, after signing some despatches, he suddenly cried out, "Je suis mort," and dropt down lifeless on the ground.

He was twice married; first to Mademoiselle Bettinger, by whom he had two children; a son, an officer in the artillery, who inherits his title; and a daughter, Madame Foucaud. He was married a second time to Madame Belleville, the widow of Vailly, by whom he had no family. He left but little fortune behind him; and two maiden sisters who lived with him depended for their support upon his friend, M. Vauquelin.

The character of M. Fourcroy is sufficiently obvious. It was exactly fitted to the country in which he lived, and the revolutionary government, in the midst of which he was destined to finish his career. Vanity was his ruling passion, and the master-spring of all his actions. It was the source of all the happiness and of all the misery of his life; for every attack, from what quarter soever it proceeded, was felt by him with equal acuteness. The sneer of the most ignorant pretender, or the most obscure paper, affected him just as much as if it had proceeded from the most profound philosopher. It is needless to observe, after this, how much he must have suffered from the various parties into which the French chemists divided themselves; all of which were more or less hostile to him, excepting the one which he himself headed. His occupations were too numerous, and his elocution too ready, to put it in his power either to make profound discoveries, or to compose treatises of great depth or originality. The changes which took place in the science of chemistry were brought about by others, who were placed in a different situation, and endowed with different talents; but no man contributed so much as Fourcroy to the popularity of the Lavoisierian opinions, and the rapidity with which they were propagated over France and most countries in Europe. His eloquence drew crowds to hear him, and persuaded his audience to embrace his opinions.

He must have possessed an uncommon facility in writing, for his literary labours are exceedingly numerous. Besides those essays which have been already noticed, he published five editions of his System of Chemistry, each of them gradually increasing in size and value; the first edition being in two volumes, and the fifth in ten. This last edition he wrote in sixteen months. It contains a vast quantity of valuable matter, and contributed considerably to the general diffusion of chemical knowledge. Its fault is the diffuseness of the style, and the want of correct references. The readers of Fourcroy's system would suppose that all the discoveries in chemistry have been made by the French, and that other nations have contributed comparatively little to the stock of chemical knowledge; whereas in reality, the very opposite is the truth. A much greater number of important chemical discoveries have been made in Britain than in France; and the British chemists have contributed prodigiously to the raising of that beautiful fabric which we at present admire.

Perhaps the best of all Fourcroy's productions is his Philosophy of Chemistry, which is remarkable for its conciseness, its perspicuity, and the neatness of its arrangement.

Besides these works, and the periodical work called Le Medicin Eclairé, of which he was the editor, there are above 160 papers on chemical subjects, with his name attached to them as the author, which appeared in the Memoirs of the Academy of the Institute, in the Annales de Chimie, or the Annales de Museum d'Histoire Naturelle, of which last work he was the original projector. As in most of these papers the name of Vauquelin is associated with his own, as the author; and as during the publication of those which appeared with his own name alone, Vauquelin was the operator in his laboratory, it is not possible to determine what part of the experiments were made by Fourcroy, and what by Vauquelin. I have been told, by a gentleman who had a good opportunity of getting information on the subject, that almost all the experiments were made by Vauquelin, but that all the papers were written by Fourcroy himself. The discoveries contained in these numerous dissertations, are of considerable importance, and relate chiefly to animal and vegetable chemistry. At the same time, it must be allowed, that Fourcroy and Vauquelin often fall into mistakes difficult to be accounted for; and that they do not, in every case, do justice to their predecessors or contemporaries, who had been occupied with the same investigations. A complete enumeration of these dissertations would probably be considered as too tedious. I shall therefore satisfy myself with pointing out some of his most important chemical discoveries and observations.

1. He repeated the curious experiments of Berthollet upon the evolution of azotic gas from animal substances. This dissertation contains very little new, and is remarkable for some striking mistakes; as for example, that azotic gas has the property of giving a green colour to vegetable blues. If such an observation was ever actually made, he must have been deceived by a portion of ammonia mixed with the azotic gas. He announced, soon after, that the air contained in the swimming bladder of the carp is azotic gas. This paper contains several absurd observations on the method of procuring azotic gas; as for example, that the black oxide of manganese gives out pure azote, if exposed to a heat below redness.

2. He analysed a green coloured mineral from Auvergne, which he found a mixture of arseniate of lead and phosphate of lead.

3. He affirmed that ammonia is decomposed by the oxides of manganese, mercury, and iron; and that these oxides, at the same time, lose either the whole or a portion of their oxygen.

4. He ascertained that the most common constituent of biliary calculi is a substance very similar in its properties to spermaceti. This substance, in consequence of a subsequent discovery which he made, during the removal of dead bodies from the burial ground of the Innocents at Paris, namely, that these bodies were converted into a fatty matter, got the name of adipocire.

5. He found that vegetable juices frequently contain a substance which coagulates when the juice is exposed to a gentle heat. This substance he considered as albumen; but Proust afterwards showed that it was, in reality, a species of gluten, and quite different in its properties from albumen.

6. He ascertained the properties of several triple salts, which magnesia, and ammonia, and an acid are capable of forming; and explained, by this discovery, the reason why magnesia is not precipitated completely from its solutions by ammonia. This paper, which appeared in the fourth volume of the Annales de Chimie, I consider as one of the best ever published by Fourcroy.

7. He published a very elaborate analysis of the quinquina, a species of bark from St Domingo, which was considered at the time as a model for vegetable analysis. It bears

the exact characters of the peculiar method followed by Vauquelin. I should suppose therefore, though nothing is said on the subject, that the experiments were contrived and executed by that eminent chemist.

8. His dissertation on the sulphate of mercury, though imperfect, contains some good observations, and facilitated the knowledge of metallic salts, which was at that time very imperfect, but has been greatly improved since. The same remark applies to his subsequent memoirs on the action of ammonia, on the sulphate, nitrate, and muriate of mercury. These papers contain some mistaken opinions, though the formation of the triple salts, which constitutes the basis of his opinion, be correct.

9. His experiments on the brain contain several valuable facts, and his opinion approaches to accuracy. The subject has been recently resumed by Vauquelin, who has published a curious dissertation on it.

10. The analysis of tears, and the mucus of the nose, by Fourcroy and Vauquelin, is valuable; though it contains some mistakes, from the too hasty application of an erroneous theory to the animal phenomena.

11. The analysis of urine, and of urinary calculi, by the same gentleman, has been much admired on the Continent, and no doubt contains many important facts; but hardly any important addition is made in it to the dissertation of Dr Wollaston on the same subject, which had been already published in the Philosophical Transactions. To this very important paper no allusion whatever is made, yet they could hardly be ignorant of it, as they quote Dr Pearson's essay on the same subject, which had been published in the same work.

12. Their experiments on the combustion of bodies in oxymuriatic acid gas, and the detonations which take place when hyperoxymuriate of potash and a combustible substance are mixed together and struck upon an anvil, are curious, though they add but little to the improvement of the theory of chemistry.

13. Their method of obtaining barytes in a state of purity, by exposing the nitrate of barytes to a red heat in a porcelain crucible, is a good one; and is by far the easiest way to procure that earth in a state of tolerable purity.

14. Their theory of the formation of sulphuric ether, by the action of sulphuric acid, is plausible; and at least as likely to be true as any other explanation which has been

hitherto offered; but they were wrong in attempting to extend that theory to the formation of ether in general. We now know that the nitric, and muriatic, and acetic ether, are formed in quite a different manner.

15. They ascertained by experiment that the three liquids, known by the names of pyromucous, pyrolignous, and pyrotartarous acids, are nothing else than vinegar holding in solution a portion of empyreumatic oil.

16. They ascertained the presence of phosphate of magnesia in the bones of all animals.

17. Their experiments upon crude platina were not so successful. They detected in it the presence of a new metal. But as they in fact confounded the two metals of Tennant, the osmium and iridium, together, all their observations were either erroneous, or so confused that it was impossible to disentangle the truth from them.

18. Their experiments on the bitter principle extracted from indigo, and the detonating property which it possesses, are curious. The subject was carried further by Hatchett and Chevreul.

19. They were unsuccessful in their attempts to detect the presence of fluoric acid in bones; though this was afterwards successfully executed by Berzelius.

20. They discovered a quantity of uncombined phosphorus in the melts of fishes. They showed likewise an analogy between the pollen of the antheræ of some flowers, and the seminal fluid of animals.

21. They detected in the common onion the presence of a considerable quantity of saccharine matter, and showed by experiment, that this saccharine matter was converted into manna by a spontaneous change which it underwent. They found, at the same time, that manna is incapable of undergoing the vinous fermentation, and, of course, that it does not yield alcohol.

22. They ascertained the properties of animal mucus, and showed that it differed from all other animal substances.

23. These, though only a small number of the chemical papers published by Fourcroy, are by far the most important. We have no means of determining what portion of each belongs to Fourcroy, and what to Vauquelin; but there is one merit, at least, which cannot be refused Fourcroy, and it is no small one. He formed and brought forwards Vauquelin, and proved to him ever afterwards a most steady and indefatigable friend. This is bestowing no small panegyric on

his character; for it would have been impossible to have retained such a friend through all the horrors of the French revolution, if his own qualities had not been such as to merit so steady an attachment. I have taken no notice of the labours of M. de Fourcroy in the chemical part of the Encyclopedie Methodique, though they are rather voluminous, because I conceive them of inferior importance to those which I have noticed.

Art. II.—*On the Action of finely-divided Platinum on Gaseous Mixtures, and its Application to their Analysis.* By William Henry, M. D. F. R. S. [*Philos. Trans.*]

Several years have elapsed since the President of the Royal Society, in the further prosecution of those researches on flame which had already led him to the most important practical results, discovered some new and curious phænomena in the combustion of mixed gases, by means of fine wires of platinum introduced into them at a temperature below ignition. A wire of this sort being heated much below the point of visible redness, and immersed in a mixture of coal gas and oxygen gas in due proportions, immediately became white-hot, and continued to glow until all that was inflammable in the mixture was consumed. The wire, repeatedly taken out of the mixture and suffered to cool below the point of redness, instantly recovered its temperature on being again plunged into the mixed gases. The same phænomena were produced in mixtures of oxygen with olefiant gas, with carbonic oxide, with cyanogen, and with hydrogen; and in the last case there was an evident production of water. When the wire was very fine, and the gases had been mixed in explosive proportions, the heat of the wire became sufficiently intense to cause them to detonate. In mixtures which were non-explosive from the redundancy of one or other gas, the combination of their bases went on silently, and the same chemical compounds were formed as by their rapid combustion.

Facts analagous to these were announced in the autumn of last year by Professor Dœbereiner, of Jena, with this additional and striking circumstance,—that when platinum in a spongy form is introduced into an explosive mixture of oxygen and hydrogen, the metal, even though its temperature had not been previously raised, immediately glows, and causes the union of the two gases to take place, sometimes silently, at others with detonation. It is remarkable, however, that platinum in this form, though so active on mixtures of oxygen and hydrogen, produces no effect, at common temperatures, on mixtures of oxygen with those compound gases, which were found by Sir Humphry Davy to be so readily acted upon by the heated wire. Carbonic oxide appears indeed, from the statement of MM. Dulong and Thenard, to be capable of uniting with oxygen at the temperature of the atmosphere, by means of the sponge; but though this is in strictness true, yet the combination, in all the experiments I have made, has been extremely slow, and the due diminution of volume has not been completed till several days have elapsed. On mixtures of olefiant gas, of carburetted hydrogen, or of cyanogen, with oxygen, the sponge does not by any duration of contact exert the smallest action at common temperatures.

It was this inefficiency of the platinum sponge on the compounds of charcoal and hydrogen in mixture with oxygen, while it acts so remarkably on common hydrogen, and also, though slowly, on carbonic oxide, that suggested to me the possibility of solving by its means some interesting problems in gaseous analysis. I hoped more especially to be able to separate from each other the gases constituting certain mixtures, to the compositions of which approximations only had been hitherto made, by comparing the phænomena and results of their combustion with those which ought to ensue, supposing such mixtures to consist of certain hypothetical proportions of known gases. It might, for instance, be expected that from a mixture of hydrogen and carburetted hydrogen with oxygen, the platinum sponge would cause the removal of the hydrogen, leaving the carburetted hydrogen unaltered. To ascertain this and a variety of similar facts, I made artificial mixtures of the combustible gases in known volumes; and submitted them, mixed with oxygen, sometimes to contact with the sponge, and sometimes with the balls made of clay and platinum, described by Professor Dœbereiner.

SECTION I.

ON THE ACTION OF FINELY-DIVIDED PLATINUM ON GASEOUS MIXTURES AT COMMON TEMPERATURES.

1. *Mixtures of Hydrogen and Olefiant Gases with Oxygen.*

When to equal volumes of olefiant gas, and an explosive mixture (which is to be understood, whenever it is so named, as consisting of two volumes of hydrogen and one of oxygen gases,) one of the platinum balls, recently heated by the blowpipe, and allowed to cool during eight or ten seconds, is introduced through mercury, a rapid diminution of volume takes place; the whole of the hydrogen and oxygen gases is condensed; but the olefiant gas is either not at all or very little acted upon. In a few experiments when the tube was narrow, and the quantity of mixed gases small, the olefiant gas escaped combustion entirely; but, in general, an eighth or tenth of it was converted into water and carbonic acid. It is difficult, however, to state the precise proportion of any gas which, when added to an explosive mixture, renders the latter insensible to the action of the balls or sponge; for much depends on their temperature when introduced into the gaseous mixture, the diameter of the containing vessel, and other circumstances, which, in comparing different gases, should be so regulated as to be equal in every case.

When the proportions of the gases are changed, so that the explosive mixture exceeds in volume the olefiant gas, there is a more decided action upon the latter, manifested by an increased production of carbonic acid. Thus, for example, the explosive mixture being to the olefiant as 2½ to 1, about one-fourth of the olefiant gas was consumed; and by increasing the proportion of the explosive mixture the olefiant gas was still more acted upon. On using oxygen sufficient to saturate both the hydrogen and the olefiant gases, the ball acted much more rapidly. In several instances it became red-hot; all the hydrogen was consumed; and the whole of the olefiant gas was changed into water and carbonic acid. In this case the use of the sponge is inadmissible, as it kindles the gases, and occasions their detonation.

2. *Mixtures of Hydrogen and Carburetted Hydrogen Gases with Oxygen.*

When carburetted hydrogen, procured from stagnant water, was added to an explosive mixture, in various proportions between equal volumes, and ten of the former to one of the latter, the action of the hydrogen and oxygen on each other took place as usual, on admitting one of the balls. When, reversing the proportion, the explosive mixture was made to exceed the carburetted hydrogen, but not more than four or five times, the latter gas was entirely unchanged. With a larger proportion of the explosive mixture, carbonic acid was always found to have been produced; but still the carburetted hydrogen was very imperfectly consumed; and fully three-fourths of it were generally found to have escaped unburned.

When to a mixture of hydrogen and carburetted hydrogen, oxygen enough was added to saturate both gases, the effect of the sponge was found to vary with the proportion of the simple hydrogen. In several cases, where the hydrogen did not exceed the carburetted hydrogen more than four times, the latter gas remained unchanged; when in larger proportion, there was a decided action upon the carburetted hydrogen. But it was much more easy to regulate the action of the balls upon such a mixture so as to act upon the hydrogen and oxygen only, than in the case of olefiant gas, which, under similar circumstances, is always more largely converted into water and carbonic acid.

3. *Mixtures of Hydrogen and Carbonic Oxide with Oxygen.*

The addition of one volume of carbonic oxide to two volumes of an explosive mixture produces a distinct effect in suspending the action of the platinum balls, and even of the spongy metal itself. The action of the gases upon each other still, however, goes on slowly, even when the carbonic oxide exceeds the explosive mixture in volume; and after the lapse of a few days the oxygen is found to have disappeared, and to have partly formed water, and partly carbonic acid. I made numerous experiments to ascertain whether the oxygen, under these circumstances of slow combustion, is divided between the carbonic oxide and the hydrogen in proportions corresponding to the volumes of those two gases. The combustible gases being in equal volumes, and the oxygen sufficient to saturate only one of them, it was found that the oxygen which had united with the carbonic oxide was to that which had combined with the hydrogen as about 5 to 1 in volume. Increasing the carbonic oxide, a still larger proportion of oxygen was expended in forming carbonic acid. On the contrary, when the hydrogen was increased, a greater proportional quantity of oxygen went to the formation of water. But it was remarkable, that when the hydrogen was made to exceed the carbonic oxide four or five times, less oxygen in the whole was consumed than before; the activity of the carbonic oxide appearing to have been diminished, without a corresponding increase in that of the hydrogen.

In cases where the proportion of the carbonic oxide to the explosive mixture was intentionally so limited that the platinum ball was capable of immediately acting upon the latter, the carbonic oxide was always in part changed into carbonic acid, the more abundantly as its volume was exceeded by that of the explosive mixture. Increasing the oxygen, so that it was adequate to saturate both gases, and causing the hydrogen to exceed the carbonic acid in volume, a speedy action was always exerted by the ball, and the whole of the combustible gases was silently converted into water and carbonic acid. The introduction of the platinum sponge into such a mixture was almost always found to produce detonation.

4. *Mixtures of Hydrogen and Cyanogen with Oxygen.*

When one of the platinum balls, after being recently heated, is introduced into cyanogen and explosive mixture in equal volumes, no apparent action takes place. With half a volume of cyanogen there is a slight diminution; and as we reduce the proportion of that gas, the action of the elements of the explosive mixture on each other becomes more and more distinct. There is not, however, as with carbonic oxide, any production of carbonic acid; but in the course of a few minutes, the inside of the tube becomes coated with a brownish substance, soluble in water, and communicating to it the same colour; having a smell resembling that of a burnt animal substance, and yielding ammonia on the addition of a drop or two of liquid potash. It was produced in too small a quantity to enable me to submit it to a more minute examination; but its characters appeared to resemble those of a product obtained by M. Gay-Lussac by mixing cyanogen with ammoniacal gas.

If oxygen be added to a mixture of hydrogen and cyanogen, in quantity sufficient to saturate both the gases, it is still necessary, in order that an immediate effect should be produced by the sponge, that the hydrogen should exceed the cyanogen in volume. A decided action then takes place; an immediate absorption ensues; fumes of nitrous acid vapour appear, which act on the surface of the mercury; and after removing the nitrous acid by a drop or two of water, and transferring the gas into a dry tube, carbonic acid is found to have been produced, equivalent in volume to double that of the cyanogen.

5. *Effect of adding various other Gases to an Explosive Mixture of Hydrogen and Oxygen.*

It had been already ascertained by Professor Dœbereiner, that one volume of oxygen diluted with 99 volumes of nitrogen is still sensible, when mixed with a due proportion of hydrogen, to the action of the sponge. Carbonic acid also, even (I find) when it exceeds the explosive mixture ten times, retards only in a slight degree the energy of the sponge. Oxygen, hydrogen, and nitrous oxide gases, when employed to dilute an explosive mixture, are equally inefficient in preventing the mutual action of its ingredients. Ammonia may be added in ten times the volume of the explosive mixture, and muriatic acid gas in six times its volume, with no other effect than that of rendering the action of the sponge less speedy.

6. *Mixtures of Carbonic Oxide and Carburetted Hydrogen with Oxygen.*

When mixtures of these gases are exposed to the sponge, the carburetted hydrogen seems to stand entirely neutral. The carbonic oxide is converted into carbonic acid, in the same gradual manner as if it had been mixed with oxygen only; and the carburetted hydrogen remains unaltered.

7. *Mixtures of Hydrogen, Carburetted Hydrogen, and Carbonic Oxide with Oxygen.*

In mixtures of these gases, it is of little consequence whether the oxygen be sufficient for the hydrogen and carbonic oxide only, or be adequate to the saturation of all three. The circumstance which has the greatest influence on the results of exposing such mixtures to the sponge, is the proportion which the simple hydrogen bears to the other gases, and especially to the carbonic oxide; for in order that there may be any immediate action, the former should exceed the latter in volume. In that case the hydrogen is converted into water, and the carbonic oxide into carbonic acid; but the carburetted hydrogen, unless the excess of hydrogen be very considerable, remains unaltered. If the proportion of hydrogen be so small that no immediate action is excited by the sponge, the ingredients of the mixture nevertheless act slowly upon each other; and after a few days the whole of the hydrogen and carbonic oxide are found to have united with oxygen, and the carburetted hydrogen to remain of its original volume.

8. *Mixtures of Hydrogen, Carbonic Oxide, and Olefiant Gases with Oxygen.*

When the oxygen, in a mixture of these gases, is sufficient to saturate the two first only, and the proportion of hydrogen is so adjusted that the action of the sponge is not very energetic, the hydrogen and carbonic oxide only are acted upon; but if the diminution of volume, which the sponge produces, be rapid and considerable, part of the olefiant gas is converted into water and carbonic acid. This effect on olefiant gas takes place still more readily if the oxygen present be adequate to the saturation of all three combustible gases.

It is remarkable, that if to a mixture of hydrogen, carbonic oxide, and oxygen, in such proportions that the sponge would act rapidly in producing combination, olefiant gas be added, the action of the gases on each other is suspended. Thus 20 measures of carbonic oxide, 31 of hydrogen, and 20 of oxygen, were instantly acted upon by the sponge; but the addition of 20 measures of olefiant gas to a similar mixture entirely suspended its efficiency. By standing fourteen days, rather more than half the carbonic oxide was acidified, and about one-twelfth of the hydrogen was changed into water, but the olefiant gas remained unaltered.

9. *Mixtures of Hydrogen, Carbonic Oxide, Carburetted Hydrogen, and Olefiant Gases with Oxygen.*

In mixtures of these four gases with oxygen, it was found by varying the proportion of hydrogen, that hydrogen and carbonic oxide are most easily acted upon; then olefiant gas; and carburetted hydrogen with the greatest difficulty. When the action of the sponge was moderate, only the hydrogen and carbonic oxide were consumed, or at most the olefiant gas was but partially acted upon. Adding more hydrogen, so as to occasion a more rapid diminution, the olefiant gas also was burned; but the carburetted hydrogen always escaped combustion, unless the hydrogen were in such proportion that the ball or sponge became red-hot.

From the facts which have been stated, it appears that when the compound combustible gases, mixed with each other, with hydrogen, and with oxygen, are exposed to the platinum balls or sponge, the several gases are not acted upon with equal facility; but that carbonic oxide is most disposed to unite with oxygen; then olefiant gas; and lastly, carburetted hydrogen. By due regulation of the proportion of hydrogen, it is possible to change the whole of the carbonic oxide into carbonic acid without acting on the olefiant gas or carburetted hydrogen. With respect indeed to olefiant gas, this exclusion is attended with some difficulty, and it is generally more or less converted into carbonic acid and water. But it is easy, when olefiant gas is absent, so to regulate the proportion of hydrogen, that the carbonic oxide may be entirely acidified, and the whole of the carburetted hydrogen be left unaltered. This will generally be found to have been accomplished, when the platinum ball has occasioned a diminution of the mixture, at about the same rate as atmospheric air is diminished by nitrous gas when the former is admitted to the latter in a narrow tube.

SECTION II.

On the Effect of finely-divided Platinum on Gaseous Mixtures at increased Temperatures.

The effect of varying the proportion of free hydrogen to the compound combustible gases, on the degree of action which is excited by the platinum sponge, will perhaps admit of being explained, by examining the facts that have been stated, in connexion with the degrees of combustibility of the compound gases under ordinary circumstances. The precise degree of temperature at which any one of them burns is not known, on account of the imperfection of our present methods of measuring high degrees of heat. It has been ascertained, however, by Sir Humphry Davy, that at a heat between that of boiling mercury and that which renders glass luminous in the dark, hydrogen and oxygen gases unite silently, and without any light being evolved; that carbonic oxide is as inflammable as hydrogen; that olefiant gas is fired by iron and charcoal heated to redness; but that carburetted hydrogen, to be inflamed, requires that the wire should be white-hot. Now this is precisely the order in which the three compound gases require hydrogen to be added to them, in order to be rendered susceptible of being acted upon by the platinum sponge; carbonic oxide being acted upon with the smallest proportion of hydrogen, olefiant gas requiring more hydrogen, and carburetted hydrogen a still larger proportion. It is extremely probable then, that the temperature, produced by the union of the hydrogen and oxygen forming part of any mixture, is the circumstance which determines the combustible gases to unite, or not, with oxygen, by means of the sponge. It was desirable, however, to ascertain the exact temperature at which each of those three gasses unites with oxygen with the intervention of the spongy platinum. For this purpose the gases, mixed with oxygen enough to saturate them, were severally exposed in small retorts containing a platinum sponge, and immersed in a mercurial bath, to a temperature which was gradually raised till the gases began to act on each other. In this way the following facts were determined.

1st. Carbonic oxide began to be converted into carbonic acid at a temperature between 300° and 310° Fahrenheit. By raising the temperature to 340°, and keeping it at that point for 10 or 15 minutes, the whole of the gas was acidified, the condensation of volume in the mixture being equivalent to the oxygen which had disappeared,

2d. Olefiant gas, mixed with sufficient oxygen, and in contact with the sponge, showed a commencement of decomposition at 480° Fahrenheit, and was slowly but entirely changed into carbonic acid by a temperature not exceeding 520° Fahrenheit. MM. Dulong and Thenard state the same change to take place at 300° Cent. = 572° Fahrenheit; but having repeated the experiment several times, I find no reason to deviate from the temperature which I have assigned.

3d. Carburetted hydrogen, exposed under the same circumstances, was not in the least acted upon by a temperature of 555° Fahrenheit, the highest to which, by an Argand's lamp, I was able to raise the mercurial bath. This, however, must have been near the temperature required for combination; for on removing the retort from the mercurial bath, and applying a spirit lamp, at such a distance as not to make the retort red-hot, a diminution of volume commenced, and continued till all the carburetted hydrogen was silently converted into water and carbonic acid.

4th. Cyanogen, similarly treated, was not changed at a temperature of 555° Fahrenheit; and on applying the flame of a spirit lamp to the tube, it produced no action till the tube began to soften.

5th. Muriatic acid gas, mixed with half its volume of oxygen, began to be acted upon at 250° Fahrenheit. Water was evidently formed; and the disengaged chlorine, acting upon the mercurial vapour in the tube, formed calomel, which was condensed, and coated its inner surface.

6th. Ammoniacal gas, mixed with an equal volume of oxygen, showed a commencement of decomposition at 380° Fahrenheit. Water was also in this case distinctly generated;

and at the close of the experiment nothing remained in the tube but nitrogen and the redundant oxygen.

I proceeded, in the next place, to examine the agency of finely-divided platinum at high temperatures, on those mixtures of gases, which are either not decomposed, or are slowly decomposed, at the temperature of the atmosphere.

When carbonic oxide and hydrogen gases, in equal volumes, mixed with oxygen sufficient to saturate only one of them, were placed in contact with the sponge, and gradually heated in a mercurial bath, the mixture ceased to expand between 300° and 310° Fahrenheit, and soon began to diminish in volume. On raising the temperature to 340°, and keeping it some time at that point, no further diminution was at length perceptible. From the quantity of carbonic acid remaining at the close of the experiment, it appeared that four-fifths of the oxygen had united with the carbonic oxide, and only one-fifth with the hydrogen. When four volumes of hydrogen, two of carbonic oxide, and one of oxygen, were similarly treated, the hydrogen, notwithstanding its greater proportional volume, was still found to have taken only one-fifth of the oxygen, while four-fifths had combined with the carbonic oxide. These facts show that at temperatures between 300° and 340° Fahrenheit, the affinity of carbonic oxide for oxygen is decidedly superior to that of hydrogen; as, from the experiments before described, appears to be the case, also at common temperatures.

But a similar distribution of oxygen, between carbonic oxide and hydrogen, does not take place, when those three gases are fired together by the electric spark. This will appear from the following table, in which the three first columns show the quantities of gases that were fired, and the two last, the quantities of oxygen that were found to have united with the carbonic oxide and with the hydrogen.

	Before Firing.			*After Firing.*	
	Measure of Carb. Oxide.	Measure of Hydrogen.	Measure of Oxygen.	Oxygen to Carb. Oxide.	Oxygen to Hydrogen.
Exp. 1.	40	40	20	6	14
2.	40	20	20	12	8
3.	20	40	20	5	15

When equal volumes of carbonic oxide and hydrogen gases, mixed with oxygen sufficient to saturate only one of them, were exposed in a glass tube to the flame of a spirit lamp, without the presence of the sponge, till the tube began to soften, the combination of the gases was effected without explosion, and was merely indicated by a diminution of volume and an oscillatory motion of the mercury in the tube. At the close of the experiment, out of twenty volumes of oxygen, eight were found to have united with the carbonic oxide, and twelve with the hydrogen—proportions which do not materially differ from the results of the first experiment in the foregoing table. At *high* temperatures, then, the attraction of hydrogen for oxygen appears to exceed that of carbonic oxide for oxygen; at lower temperatures, especially when the gases are in contact with the platinum sponge, the reverse takes place, and the affinity of carbonic oxide for oxygen prevails.

Extending the comparison to the attraction of olefiant and hydrogen gases for oxygen at a red heat, I found that when six volumes of olefiant, six of hydrogen, and three of oxygen were heated by a spirit lamp till the tube softened, a silent combination took place as before; all the oxygen was consumed, but only half a volume had been expended in forming carbonic acid, which indicates the decomposition of only one quarter of a volume of olefiant gas. On attempting a similar comparison between carbonic oxide and olefiant gas, by heating them with oxygen in the same proportions, the mixture exploded as soon as the glass became red-hot, and burst the tube.

The property inherent in certain gases, of retarding the action of the platinum sponge, when they are added to an explosive mixture of oxygen and hydrogen, is most remarkable in those which possess the strongest attraction for oxygen; and it is probably to the degree of this attraction rather than to any agency arising out of their relations to caloric, that we are to ascribe the various powers which the gases manifest in this respect. This will appear from the following table the first column of which shows the number of volumes of each gas required to render one volume of an explosive mixture of hydrogen and oxygen uninflammable by the discharge of a Leyden jar; while the second column shows the number of volumes of each gas necessary in some cases to render one volume of an explosive mixture insensible to the action of the sponge, and in other cases indicates the number which may be added without preventing immediate combination. In the first column, the numbers marked with an asterisk were determined by Sir Humphry Davy; the remaining numbers in that column, and the whole of the second, are derived from my own experiments.

1 vol. of Explosive Mixture was rendered incapable of being inflamed by electricity when mixed with			**Effect of adding the same gases to 1 vol. of Explosive Mixture on the action of the sponge.**
*	About 8	vol. of Hydrogen	not prevented by many vol.
	6	Nitrogen	ditto.
*	9	Oxygen	not prevented by 10 vol.
*	11	Nitrous Oxide	ditto.
	1.5	Cyanogen	prevented by 1 vol.
*	1	Carb^{ed}. Hyd.	not prevented by 10 vol.
	4	Carb^{o}. Oxide	prevented by ½ vol.
*	0.5	Olefiant Gas	prevented by 1·5 vol.
*	2	Muriatic Acid	not prevented by 6 vol.
	2	Ammonia	not prevented by 10 vol.
	3	Carbonic Acid	ditto.

From the foregoing table it appears that carbonic oxide produces the greatest effect, in the smallest proportion, to an explosive mixture of oxygen and hydrogen, in preventing the action of those gases on each other, when exposed to the sponge at temperatures below the boiling point of mercury. In general those gases which either do not unite with oxygen, or unite with it only at high temperatures, have little effect in restraining the efficiency of the sponge. There is an apparent exception, however, in cyanogen, which it would require more research than I have yet had time to devote to an object merely collateral, to reconcile it, if it be capable of being reconciled with the general principle.

From the fact that carbonic oxide, olefiant gas, and carburetted hydrogen, when brought to unite with oxygen by means of the platinum sponge, assisted by heat, undergo this change at different temperatures, it seemed an obvious conclusion, that by exposing a mixture of those gases with each other and with oxygen to a regulated temperature, the correct analysis of such mixtures might probably be accomplished. Mixtures of two or more of the combustible gases were therefore exposed, in contact with oxygen gas and the platinum sponge, in tubes bent into the shape of retorts, which were immersed in a mercurial bath. This bath was gradually heated to the required temperatures, and by proper management of the source of heat was prevented from rising above that degree.

1st. By subjecting 25 measures of carbonic oxide, 15 of olefiant gas, and 57 of oxygen, in contact with the sponge, to a heat which was not allowed to exceed 350° Fahrenheit till the diminution of volume ceased all the carbonic oxide was converted into carbonic acid, and the olefiant gas remained in its original volume.

2d. By exposing in a similar manner 20 measures of carbonic oxide, 21 of carburetted hydrogen, and 36 of oxygen, to a temperature below 400° Fahrenheit, the carbonic oxide was entirely acidified; and on washing out the carbonic acid by liquid potash, the carburetted hydrogen was found unaltered, mixed with the redundant oxygen.

3d. A mixture of 10 measures of olefiant gas, 10 of carburetted hydrogen, and 58 of oxygen, being heated in contact with the sponge to 510° Fahrenheit, the olefiant gas was silently but entirely changed into carbonic acid, while the carburetted hydrogen was not at all acted upon.

4th. By acting with the sponge upon 42 measures of carburetted hydrogen, 22 of carbonic oxide, 22 of hydrogen, and 28 of oxygen, first at a temperature of 340° Fahrenheit, which was raised gradually to 480°, all the carbonic oxide was changed into carbonic acid, and all the hydrogen into water; but the carburetted hydrogen remained undiminished in quantity, and was found, after removing the carbonic acid, mixed only with the redundant oxygen. In this experiment the diminution of volume had continued some time before there was any perceptible formation of water, the attraction of carbonic oxide for oxygen appearing to prevail over that of hydrogen. The same precedency in the formation of carbonic acid is always apparent when carbonic oxide and hydrogen, mixed even with oxygen enough to saturate both gases, are raised to 350° Fahrenheit.

By thus carefully regulating the temperature of the mercurial bath, the action of oxygen upon several gases (carbonic oxide, olefiant, and carburretted hydrogen, for example,) may be made to take place in succession; and by removing the carbonic acid formed at each operation, it may be ascertained how much of each of the two first gases has been decomposed. The carburetted hydrogen indeed always remains unchanged, and its quantity must be determined by firing it with oxygen by the electric spark. If hydrogen also be present, it is difficult to prevent the olefiant gas from being partially acted upon; but this is of little consequence, as I had shown that it is easy to remove that gas in the first instance by chlorine. It may be remarked, that this method of operating on the aeriform compounds of charcoal gives more accurate results than rapid combustion by the electric spark, being never attended with that precipitation of charcoal which is often observed when the gases are exploded with oxygen. A regulated temperature also effects the analysis of such mixtures much more correctly than the action of the sponge or balls, because in the latter case the heat produced is uncertain; and though sometimes adequate to the effect, yet there is always a risk that it may exceed or fall short of that degree which is required for the successful result of the analytic process.

From the facts which have been stated, I derive a method of obtaining carburetted hydrogen gas perfectly free from olefiant gas, hydrogen, and carbonic oxide, and mixed only with a little oxygen, which, had it been necessary to my purpose, might also have been separated. The early product of the distillation of pit-coal was washed with a watery solution of chlorine, and afterwards with liquid potash to remove a little chlorine that arose into the gas from the solution. The residuary gas was next heated with one fourth of its volume of oxygen at the temperature of 350° Fahrenheit, in contact with the sponge, which converted the carbonic oxide into carbonic acid, and the hydrogen into water. The carbonic acid being removed by liquid potash, there remained only the carburetted hydrogen, the redundant oxygen, and a very minute quantity of nitrogen introduced by the latter gas. Hitherto I have prepared this gas only in a small quantity; but it would be easy to extend the scale of the operation, and to remove the excess of oxygen by obvious methods.

SECTION III.

Application of the Facts to the Analysis of Mixtures of the combustible Gases in unknown Proportions.

At an early period of the investigation described in the first section, I proceeded to apply the facts of which I was then possessed, to the analysis of a mixture of gases in unknown proportions. For this purpose, I caused a quantity of gas to be collected from coal, by continuing the application of heat to the retorts two hours beyond the usual period and receiving the gas into a separate vessel. Gas of this quality was purposely chosen, because from former experience I expected it to contain free hydrogen, carbonic oxide, and carburetted hydrogen, but no olefiant gas; the production of which is confined to the early stages of the process. After washing it, therefore, with liquid potash, to remove a little carbonic acid, and ascertaining its specific gravity, when thus washed, to be 308, I proceeded at once to subject it to the new method of analysis.

Having ascertained by a previous experiment with Volta's eudiometer that 10 volumes of the gas required for saturation 9 volumes of oxygen, I mixed 43 measures with 43 of oxygen (= 41 pure), and passed a platinum ball, which had been recently heated, into the mixture. An immediate diminution of volume took place, attended with a production of heat and formation of moisture. The residuary gas, cooled to the temperature of the atmosphere, measured 43·5 volumes. Of these, 4·5 were absorbed by liquid potash, indicating 4·5 carbonic acid, equivalent to 4·5 carbonic oxide; the rest, being fired in a Volta's eudiometer, with an additional quantity of oxygen, gave 11 volumes of carbonic acid; the diminution being 22 volumes, and the oxygen consumed 22 also—circumstances which prove that 11 volumes of carburetted hydrogen were consumed by this rapid combustion. But of the loss of volume first observed (viz. 86 — 43·5 = 42·5), 2·25 are due to the carbonic acid formed; and deducting this from 42·5 we have 40·25, which are due to the oxygen and hydrogen, converted into water; and 40·25 × $\frac{2}{3}$ = 26·8 shows the hydrogen in the original gas. But the sum of these numbers (26·8 + 4·5 + 11) being less by 0·7 than the volume of gas submitted to analysis, we may safely consider that fraction of a measure to have been nitrogen. The composition then of the mixture will stand in volumes as follows:

Hydrogen	26·8	62·32
Carbonic oxide	4·5	10·50
Carburetted hydrogen . .	11·0	25.56
Nitrogen	0·7	1·62
	43.0	100.0

On calculating what should be the specific gravity of a mixture of gases in the above proportions, it was found to be 303; which coincides as nearly as can be expected with the actual specific gravity of the gas submitted to analysis, viz. 308. To place the correctness of the results beyond question, I mingled the gases in the above proportions, and acted on the artificial mixture in the same manner as on the original gas; when I had the satisfaction to find that the analytical process again gave the true volumes with the most perfect correctness for the hydrogen and carbonic oxide, and within the fraction of a measure for the carburetted hydrogen. Notwithstanding this successful result, which was twice obtained, I should still prefer, for the reason which has been stated, having recourse to a temperature carefully regulated, for the analysis of similar mixtures, in all cases where the hydrogen is in moderate proportion, and where great accuracy is desirable. Whenever (it may again be remarked) olefiant gas is present in a mixture, it should always be removed by chlorine before proceeding to expose the mixture to the agency of the spongy metal.

It can scarcely be necessary to enter into further details respecting methods of analysis, the application of which to particular cases must be sufficiently obvious from the experiments which have been described on artificial mixtures. The apparatus required is extremely simple, consisting, when the balls are employed, of graduated tubes of a diameter between 0·3 and 0·6 of an inch, or, when an increased temperature is used, of tubes bent into the shape of retorts, of a diameter varying with the quantity of gas to be submitted to experiment, which may be from half a cubic inch to a cubic inch or more. These when in use may be immersed in a small iron cistern containing mercury, and provided with a cover in which are two holes, one for the tube and the other for the stem of a thermometer, the degrees of which are best engraved on the glass.

By means of these improved modes of analysis, I have already obtained some interesting illustrations of the nature of the gases from coal and from oil. I reserve, however, the communication of them till I have had an opportunity of pursuing the inquiry to a greater extent, and especially of satisfying myself respecting the exact nature of the compound of charcoal and hydrogen, discovered some years ago by Mr Dalton, in oil gas and coal gas, which agrees with olefiant gas in being condensible by chlorine, but differs from it in affording more carbonic acid and consuming more oxygen.

Art. III.—*Facts proving the Efficacy of Sir H. Davy's Method of protecting the Copper of Ships by Electrochemical Action.* Extracted from the Letters of a Correspondent, and Dr Stewart Traill. [*An. Philos.*]

1. The *Carnebrea Castle*, an Indiaman, belonging to Messrs Wigram, of 650 tons burden, was protected last spring by a

quantity of iron in four portions, two on the bow and two on the stern, equal to form $\frac{1}{160}$ to $\frac{1}{110}$ part. She has since made the voyage to India, and was for some time in the Ganges.

She appeared bright and clean during the whole of the voyage out and home; some mud collected on her bottom in the Ganges; but immediately disappeared when she began to sail. She was put into dry dock about a fortnight ago, and her bottom examined by Sir H. Davy, the proprietors, and various other persons. *Every part* of her bottom was bright and clean, without a single adhesion of any kind, and as far as could be judged from the smoothness and appearance of the copper, it had not been at all worn by any chemical corrosion. The iron, which was about an inch and a half in thickness, is considered a sufficient protector for two voyages more.

2. The *Elizabeth* yacht, belonging to the Earl of Darnley, was protected by two pieces of malleable iron in the stern, in May last, equal to about $\frac{1}{125}$ of the surface of the copper. After being employed in sailing during the summer, she was examined in November, when her bottom was found free from adhesions of any kind, and apparently untouched. The copper was bright, and even the nails not tarnished. In the course of the summer a few small barnacles had adhered to the rust of the iron, which were easily and immediately washed off; but no weed or shell-fish had ever fixed on the copper, which appeared in the same state as when she left the dock.

The following examples we owe to the kindness of Dr Traill:

The ship *Huskisson*, belonging to Mr Horsfall, was lately in dock after a voyage to and from Demerara, where she lay some weeks, in a river remarkably favourable to the adhesion of parasitical animals and weeds; yet, when I examined this vessel, her copper appeared perfectly clean, as far as it could be seen, when she was purposely *set by the stern*, in unloading, in order to show her copper at the *bows* as low as possible. The captain stated that, before coming into port, while yet in clear water, he had seen her bottom even to the keel; and it seemed to him quite clean. This ship was *defended* by two bars of malleable iron, bolted along the sides of her keel by copper fastenings, which covered about $\frac{1}{90}$ of the surface of her copper.

The *Elizabeth*, a vessel defended exactly in the same manner, with metals in the same proportions, had made the same voyage. Both had been newly coppered when they last left

Liverpool; and the Elizabeth's copper appeared equally clean as that of the *Huskisson* when unloaded; but as she did enter a *graving* dock, we cannot absolutely say whether she was quite clean, especially as the copper of the Dorothy (about to be mentioned) appeared equally so, until she was seen in the graving dock, when the flat part of her bottom was found to be quite covered with barnacles. The copper of the *Huskisson*, there is reason to believe, was *perfectly clean*, as was proved in the next case.

The ship *Dee*, a very large vessel belonging to my relative, Mr Sandbach. This ship was newly coppered about twelve months ago, and a bar of malleable iron, about $\frac{7}{8}$ of an inch thick, and three inches broad, was fastened on each side of the keel by *iron* spikes. It covered about $\frac{1}{90}$ of the surface of her copper. Since that period she has made two voyages to Demerara, and was, at the conclusion of the last, put into a graving dock, when her copper was found perfectly free from corrosion, and there were scarcely any substances adhering to it, except a very few minute barnacles, near the keel *fore* and *aft*. This case shows that *over defence* was not the cause of the foulness of the bottom of the *Tickler;* for both in this vessel and in the *Huskisson*, the proportion of iron to the copper was greater than in that ship. The iron spikes employed to fasten the iron on the keel of the Dee, were so much corroded, as to endanger the falling off of the bars; copper nails are, therefore, to be preferred.

The Dorothy.—Dr Traill states, that the following particulars of the Dorothy's outfit and return, were communicated to him by his intelligent friend, Mr Horsfall, one of the owners of the ship in the beginning of May:

"The *Dorothy* had been coppered about a year, and had made one voyage to Bombay and back to this port, when, in May, 1824, it was determined to place bars of iron four inches broad, and one inch thick, along her keel, covering about $\frac{1}{70}$ part of the copper, in the expectation that the iron would at least so far preserve the copper from corrosion that it might be permitted to run a second voyage to India without being renewed, which can seldom be done with perfect safety. The iron extended from one end of the keel to the other, and was fastened on with copper nails with large heads. The Dorothy, thus defended, sailed again for Bombay in June, and returned to Liverpool about a month since. She was put into the graving dock yesterday, (May 3,) and an examination of her bottom took place as soon as the water had left her.

The copper appeared no more reduced than at the termination of the first voyage. The iron was diminished generally about ¾ of an inch in breadth, and from ¼ to ½ an inch in thickness. At the ends of the vessel, for about two or three feet, the iron was much more reduced than at any other part. It was covered with the usual rust, not at all resembling cast iron, under similar circumstances. The flat of the ship's bottom from end to end, and from six to eight feet in breadth, was full of *fleshy barnacles*, (lepas anatiferà) of uncommon length, and a few of the large hard shell species (balanus tintinnabulum)."*

What remains of the iron is still considered as sufficient protection for a third voyage to India, and "it appears only to be necessary to drive the large copper nails up a little to secure the iron bars for the next voyage."

Note by Dr Traill.—We remarked that the specimens of the *lepas anatifera* were considerably larger on the *starboard* than on the larboard side of the ship. On noticing this to the captain, he informed us that the *larboard* had been the *lee* side of the vessel, almost constantly during the passage to Europe, and consequently most deeply immersed in the water—a circumstance in the economy of these animals not unworthy of notice.

It is evident that in all these last cases, particularly in the ship Dorothy, the proportion of iron has been too large, and the quantity of calcareous earth on the bottom of this ship proves that the electro-negative action has been in excess.

C.

ART. IV.—*On the Transportation of Fish from salt to fresh Water.* By J. MACCULLOCH, M. D. F. R. S., &c. [*Journ. Roy. Inst.*]

You expressed a desire to know the progress which has been made in the transplantation of fish from salt to fresh water, since the period at which I communicated the paper on that subject to your Journal. Mr Arnold, who has carried on these experiments, at my wish, with great zeal, has succeeded in adding many more to the list, and, both in respect to the physical fact, and to the question of economy, the success has been far greater than any one was willing to believe.

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* Sulphuric acid was used to loosen and detach the shells.

The list of the additional fish will be seen by comparing that which is appended to this letter, with the former one; and as the subject has excited considerable attention, you will perhaps not object to a statement which may attract even more, by presenting, in the form of a prospectus, the essential facts and arguments. It is only by placing them in this form that they are very likely to produce the effect which appears desirable. I may now, however, subjoin some remarks which could not well find their place in such a statement, and which have been the result of more experience and attention. It is certain that the flavour of every fish which has yet been tried has been improved, and I can vouch for the superiority of the basse, the mullet, the loach, the atherine, and the sole, from the pond, to those from the sea. This might be expected, for it is what happens notedly with respect to oysters.

The sole becomes twice as thick as a fish of the same size from the sea, and its skin also becomes extremely dark, or nearly black.

The plaise also increases materially in thickness, and loses its spots. In some cases, it appeared three times as thick as in the sea. The basse also turns much thicker, and improves in delicacy.

The mullet almost ceases to grow in length, but enlarges in breadth, and presents a much deeper layer of fat.

Crabs and prawns have found their own way into the pond, as have loaches, and some other small fish; and while, formerly, there were none of the former two, the water is now absolutely swarming with them. Thus also, apparently, the eels have multiplied; as it is now easy to take a cartload at once, where formerly a dozen or two was a large capture. I have thus, also, more distinctly ascertained, and to the satisfaction of Cuvier, who had been unwilling to admit it, that there are two species of fresh water eel, distinguished by the comparative acuteness and breadth of the nose.

I have lastly to add an observation inadvertently omitted in the former communication, which may be used as an *à priori* argument for the possibility of this transplantation. It is, that oxygen is much more easily disengaged from fresh than from salt water. Consequently, the act of respiration ought to be easier in the former than in the latter; and, therefore, it is not to be presumed, as it has been, that sea-fish cannot respire fresh water.

As I have given the shad without its Linnæan name, I think it right to add, that our shad is yet unnamed; because the

Clupea Alosa is the Alose of the French, common in the Seine, and on the other coast of Normandy; a fish as good as our own shad is detestable, and a decidedly different species of this troublesome and ill understood genus. If I have given the vulgar term rock-fish, it is because I wish to reject the term urasse, for the present, as it stands a species; whereas the whole of this genus (labras) is still in extreme confusion, and in one, which I hope to aid in rectifying, with the assistance of Cuvier's materials and our own species.

Prospectus of a Plan for Preserving and Rearing Fish for the London Market.

From various observations and experiments, of which evidence is subjoined, it has been found that sea-fish will live and thrive, and also breed, in ponds and enclosures; and, with regard to many, it also appears that it is indifferent whether the water is salt, or fresh, or brackish, or alternately fresh and salt.

It is also found that they may be fed in such enclosures, if necessary, as our domestic animals are; but that if sufficient numbers and kinds are placed together, they feed each other without requiring further care.

It is further observed, that every, or almost every species, improves in flavour and quality, as oysters are known to do, under transplantation.

It is well known that, of all the fish brought to market, a very small proportion is in good condition, the rest being apparently ill fed; and hence the number of bad fish so well known to fishmongers.

It is much better known that, from bad weather, or other causes, the supply of the market is very irregular. Thus the public suffers when the supply is short, and the merchant when there is a glut. It is not uncommon for a glut to come in London when the town is empty; and, on the contrary, for it to want fish when full.

The proposed plan, if executed, would bring the fish within our own power, to be taken alive when wanted, and, from being better fed, in greater perfection and more uniformly good. It would be like taking stalled oxen, instead of wild Scotch cattle. It would also enable the merchants to regulate the supply by the demand, and thus to satisfy better the public and themselves. The trade would be steady instead of precarious; as the prices to the public would also be. It would be conducted, directly between the public and the grower, or with the intervention of one retailer only, and thus a heavy cause of complaint be removed. Lastly, the public would be always sure of fish, and it would be sure of them at moderate prices.

Such are the proposed advantages. As far as the species that breed in such confinement, the fish would reproduce themselves, or a pond of this nature would resemble a common fish-pond. For those that will not, if there are any such, the ponds would be mere repositories for keeping alive, till there was a demand, the fish brought into them from the sea. They would also be feeding-places, allowing bad fish to improve. Thus far, the fisheries would go on as before, and the fishing-trade would consequently not be injured. It would be increased, on the contrary; because, with a better-regulated market and more moderate prices, the consumption would be augmented.

There can be no objection, therefore, on the score of injury to the fisheries. The plan is, virtually, one to preserve fish alive after being taken, instead of suffering them to waste, to render them better in quality, and to supply them more regularly.

The plan, therefore, is, to enclose, in any convenient part of the Thames, (since the quality of the water is proved to be indifferent) a space sufficient for the purpose. A dock, or an excavation in the nature of one, would be unnecessary, as the water itself, in many places not navigable, might be enclosed by a palisade. In this, the fish would be received from the fishermen, by means of well-boats, alive. Those which chanced to die would become the food of others. Many would breed, as they have been found to do, and thus also produce food. But they might also be fed, by means of butcher's offal, or other matters easily procured in a great city, as was the practice of the ancient Romans.

From the enclosure, the fish would be taken by nets, the kinds in demand, and the quantity, selected, and the bad fish also returned for improvement. A steam-boat would supply them to London daily, and to any market which might be established; and they might even be brought up alive, so that the unsaleable ones would not be lost.

The only capital required to be sunk, or advanced, would be in purchasing and enclosing a tract of water, and in the general establishment; possibly in stocking the pond. It could not be very large; but no estimate is now pretended to be given, nor any place pointed out; that needs not, however, be very near to London, as a steam-boat would approximate any distances. After this, the fish would be purchased from the fishermen by contract; and the establishment beginning to sell, would then pay its way.

The details of evidence in support of the practicability of this scheme, are the following:

There are three or four sea-ponds in Scotland where fish are thus kept; one in Orkney, belonging to Mrs Stewart, one on the Firth of Forth, belonging to Sir Robert Preston; and one in Galloway, belonging to Mr Macdowall.

On the Greek coast of the Adriatic, at Missolonghi and elsewhere, the same has been practised from immemorial time. It is the current practice also of Bermuda, where the inhabitants subsist chiefly on fish.

These are sea-ponds, as the water is salt. But in Sicily, from the most ancient times also, the natives transport lobsters and crabs to a fresh-water and muddy lake, for the purpose of improving them, as they also do mullet.

With respect to fresh waters, we have evidence of the power of keeping and improving sea-fish in them, from the practice of the ancient Romans. From the testimony of Columella, and the other writers, "de Re Rustica," it was the practice of the Roman farmers, in the earliest days of the Republic, to go down to the sea and bring up the spawn of sea-fish to the fresh water lakes of Rome, where they multiplied and improved. It was a branch of farming. It became the amusement and luxury of the rich and great in the times of imperial Rome; enormous establishments of this nature were formed, and the fish were often fed at an expense which, as well as the value of the ponds, proves the great extent of these repositories.

Lastly, this plan has been recently put to the test under the direction of the writer of this note, in Guernsey, by Mr Arnold. In a pond of about four acres only, many sea-fish, which will be found in the following list, are now thriving, and all those which have had sufficient time have propagated; all have improved in quality, and many very remarkably. This pond was at first worthless, containing only a few eels; at present it produces a large rent, and can supply the market when the weather prevents the boats from going out. It is remarkable also, that, since the introduction of the sea-fish, the eels have multiplied a thousand fold, so as themselves to produce a considerable revenue. This proves that fish may be fed, merely by bringing different kinds together, as is the case in nature. It may be added, that the evidence from this pond is peculiarly satisfactory, as far as relates to the indifference which sea-fish possess as to the quality of the water. Being embanked from the sea, and receiving an insufficient supply of fresh water in summer, it varies, so that while it is perfectly fresh in winter, it is nearly salt in very dry weather, and brackish in various degrees at intermediate periods. Here also, it is remarkable, that while the larger fishes have been placed there, many of the smaller ones, which formerly showed no such desire, have introduced themselves through crevices in the sea wall, and that it is, in particular, crowded with crabs and prawns.

It is now necessary to subjoin a list of the fishes which belonging naturally to the sea, have been found to live in fresh waters. Some of these have been forcibly introduced, others seek it for themselves. If the list is still limited, it is because the rest have not been tried; for no fish on which the experiment has been properly tried, has failed. When they have failed, it is because they were previously injured, or nearly killed, in the taking or the transportation. The star indicates those which have been forcibly naturalized in Mr Arnold's or some other pond.

Conger
Torsk
Sprat
Shad
Alose (of the French) clupea alosa
Greater Lamprey
Lesser Lamprey
Stickleback
Cottus quadricornis
Mullet
* Plaice
Flounder
Red Flounder, pleuronectes roseus.
White Whale
* Horse Mackerel
Pollack
Prawns
Cod

* Basse
Loach
Red Loach
* Smelt
* Atherine
* Rock-fish
* Cuckoo-fish
* Old Wife
* Sole
* Turbot
Sand Eel
Rockling
Whiting Pout
Mackerel
Herring
Crabs
* Oysters
* Muscles
Shrimps

There appears no reason why turtle should not also be cultivated, whether they would breed or not. The peacock, pintado, pheasant, and common fowl, are the natives of hot climates, and have long been naturalized to cold ones; and there is far less difference between the temperatures of the water in different climates than between those of the air. An excellent turtle has been taken in the Tamar at Saltash, after an unknown length of residence.

ART. V.—*On the Mode of obtaining Silicium, and on the Characters and Properties of that Substance.* By M. BERZELIUS.* [*Phil. Mag.*]

Decomposition of Fluate of Silica by Potassium.

WHEN we read the description of the experiments made by MM. Gay-Lussac and Thenard on the silicated fluoric acid and potassium, we cannot doubt that they decomposed the acid in the circumstances which they describe. The potassium burns in the gas and condenses it; a brown matter is formed, which, boiled in water and dried, burns in oxygen gas with the evolution of silicated fluoric gas, and leaves as a residuum a white earthy matter. In applying myself to these researches, I looked upon the reduction of the fluoric acid and of the silica as so certain, that I thought it only necessary to examine the composition of the product obtained, in order to exhibit the subject in a clear light. The experiment of MM. Gay-Lussac and Thenard gave me the same results as they obtained, with this difference, that the mass when burnt in oxygen gas was not white, and that it had preserved its primitive colour without any remarkable change. I expected to find some fluate of silica and potash, but the concentrated sulphuric acid did not disengage any trace of fluate of silica; and although carried to ebullition, it did not produce any change in the substance. Nor was the substance attacked by any acid but the fluoric, which separated the silica, and left a deep brown matter, insoluble in acids, and which did not burn on exposure to the action of fire. Was this the radical of the fluoric acid, or that of silica, or a combination of both?

To procure this substance in greater quantity, I proceeded in the following manner; I introduced into a glass retort of the capacity of about ten cubic inches a small vessel of porcelain on which was a piece of potassium of the size of a large nut. After having rapidly exhausted the retort, I let in some silicated fluoric gas, from a reservoir over mercury, and I applied the heat of a spirit-lamp. The potassium at first became white, but afterwards it became browner and browner, and at last as black as a coal: it was not long in becoming inflamed, and it burnt with a large flame of a deep-red colour, not however intensely; whilst the mercury rose rapidly in the reservoir, on account of the absorption of the silicated fluoric gas. As soon as the combustion ended, a vacuum was made in the apparatus, in order to prevent the formation of the fluate of silica and potash, and it was then left to cool. The product was a hard, porous, agglutinated, and deep-brown mass, which did not change in the air, but which like manganese, emitted the smell of hydrogen when it was pressed between the fingers or when breathed upon. In the retort, around the porcelain vessel, a light powder of a yellow-brown was remarked, which was preserved. The burn mass when thrown into water immediately produced a copious evolution of hydrogen gas. The water dissolved much fluate of potash, and the brown mass was precipitated in the form of a chestnut-brown powder, slowly evolving gas. The alkaline solution was descanted and replaced by pure water. The evolution of gas visibly diminished; and the water having again been renewed after some time, it entirely ceased; so that the brown powder did not decompose the water, even at the temperature of ebullition. The solution obtained by boiling it being very acid, it was boiled with fresh portions of water, until no signs of acidity were manifested, and the solution afterwards passed through the filtre. The water was saturated with fluate of silica and potash, and the brown powder was washed as long as the water left the slighest residuum by evaporation. The powder, being dried, was of a chestnut brown, and evidently contained heterogeneous parts of a brighter colour. The brown matter which was deposited on the glass during the combustion of the potassium was much more homogeneous. Water did not disengage hydrogen by its action, but quickly became acid. This powder was washed with the same care as the preceding.

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* From the *Annales de Chimie et de Physique*, tom. xxvii. p. 337.

Now, for the purpose of determining the change undergone by the brown matter during combustion, it was first perfectly dried and heated to a dull red in a current of hydrogen gas, and, after being weighed, exposed in a proper apparatus to a current of oxygen gas. As soon as the air of the vessel seemed to be displaced by oxygen gas, the brown matter was heated with a spirit-lamp; it immediately took fire, and burnt vividly for some time, having a pale blue flame on its surface. The remaining oxygen being passed into barytic water, rendered it very turbid, and gave a precipitate which entirely dissolved with effervescence in muriatic acid, and which, consequently, was carbonate of barytes. The burnt mass was greatly diminished in bulk, but was of much the same colour as before. Its weight was augmented scarcely half a hundredth part. There was not perceived either in the glass balloon where the combustion took place, or in the glass conducting tube, the least indication of the presence of fluoric acid; and this showed plainly that this acid was not produced by the combustion of the brown matter; and that the presence of it in the experiments of MM. Gay-Lussac and Thenard, as also in the first which I made, arose from the circumstance that the brown matter contained fluate of silica and potash, which was decomposed by the heat of the combustion, and gave out silicated fluoric acid; this result likewise deprives us of the hope of learning by these means the composition of fluoric acid; but it is not less interesting, since it seems to show that the pulverulent brown matter is really *silicium.* Its combustion in oxygen, with the production of carbonic acid, without much increase of weight, was now not so difficult to understand, since it is a property of the oxides which contain three atoms of oxygen, that their quadri-carburet burns without increase of weight. But whence comes this carbon? how is it combined with the silicium? I thought at first that it might be attributed to the presence of the oil of naphtha in which the potassium was preserved, and I repeated the experiment with potassium which had not been in this oil. The result was the same. I then began to think that this metal might contain carbon; for it had been prepared after the advantageous process lately given by Brunner, which consists in distilling at a high temperature a mixture of carbonate of potash and of carbon in a vessel of forged iron. The potassium, distilled in a glass vessel, accordingly left a coally residuum, which, by treatment with water, yielded some potash and much carbon. In repeating the experiment of the combustion of potassium in silicated fluoric gas [employing the metal thus purified], the powder was not so brown, and when burnt in oxygen acquired an increase of 40 per cent without producing carbonic acid. Its colour, however, was much the same as before combustion. But this circumstance will no longer appear astonishing, if we suppose, either that the silicium may take in its combustion a lower degree of oxidation, or that, as happens with boron, the silica which is formed prevents the entire combustion of the silicium. The residuum of the combustion, treated by fluoric acid, gave silicated fluoric gas, and its colour became much darker. Thrown on a filter, well washed and dried, it was pure *silicium.*

Description of Silicium, and of its Chemical Relations with other Bodies.

Obtained in the manner just related, silicium is of a dark nut-brown colour, without the least metallic lustre. When rubbed with steel it does not give any bright streak, and resists the friction as an earthy body does. It is incombustible in atmospheric air and in oxygen; it does not undergo any change in the flame of the blowpipe, and appears to belong to the class of the most infusible bodies. These properties seem opposed to what I have already said of the combustion of silicium, which is easily accomplished when we employ that substance as obtained immediately after its reduction by potassium. This difference in its combustibility is extremely remarkable. It does not depend on an effect attributable to the fluoric acid; for, if before burning the silicium it is treated with fluoric acid, a portion of silica will be extracted from it, which had been separated from the fluoric acid by the potash produced by the oxidation of the potassium by the air, before the experiment could be made; and the acid dissolves besides, especially by means of heat, a portion of silicum, with evolution of hydrogen gas. The silicium which then remains after filtering and washing inflames and burns with vivacity in air and in oxygen. Nor can this combustibility result from a residuum of potassium; for after combustion neither fluoric acid nor fluate of silica and potash can be extracted from the product; it appears to me to be owing more probably to a portion of hydrogen combined with the silicium; for, if the silicium be burnt in oxygen, even after having been heated in hydrogen or in a vacuum, water will be formed, but in very small quantity relatively to the great capacity of saturation of the silica. The silicium which is obtained when the brown mass procured with potassium is thrown into water is, after that, a *hydruret of silicium* or a *siliciuret of hydrogen;* the reduced mass is a *siliciuret of potassium*, which the water decomposes; the potassium is converted into potash and dissolved; the greater part of the hydrogen is disengaged, and the smaller part takes the place of the potassium in combining with the silicium. If the hydruretted silicium is put into an open platinum crucible, and slowly heated until redness commences, and then after being covered carried to a white-red heat, the silicium loses its combustibility; and treated with fluoric acid, which now dissolves no more of it, becomes perfectly pure, without sustaining the great loss which takes place when we begin by burning it.—When the hydruretted silicium is very quickly brought to a red heat, it takes fire, because the hydrogen cannot burn at a lower temperature without inflaming the silicium at the same time. If the silicium does not burn completely in this case, that circumstance does not arise from the production of an inferior degree of oxidation, but from the access of oxygen being prevented by the silica formed. Besides the loss of hydrogen which the silicium sustains at an elevated temperature, it undergoes a further change; it loses the property of dissolving in fluoric acid, is contracted in bulk, and takes a darker colour. This circumstance has certainly as great an influence in diminishing its combustibility as the loss of its hydrogen. In the state of least condensation in which it is obtained, when just separated from the potassium by means of water, it may be compared, as to its combustibility, to the porous hydrogenated charcoal of lint, which takes fire by the sparks of a steel, but which loses this property after having been exposed to an elevated temperature. The incombustibility of silicium is besides so great, that the small quantity of it which remains on the filters may be found by burning them and treating the ashes with fluoric acid.

Silicium, even when dry, stains, and strongly adheres to the glass vessels in which it is kept. When it is treated with fluoric acid, the liquid is covered with a little pellicle, which envelopes, as fat oils do, each drop that is taken from it. This pellicle rises on the sides of the vessels so long as they are damp, and then appears, by refraction, of a clearer colour than the silicium which is under the liquid.

Silicium is a non-conductor of electricity. That which has become incombustible by its exposure to a strong heat does not undergo any change, if, whilst red-hot, chlorate of potash is thrown upon it. It is not attacked by saltpetre until the temperature is raised sufficiently to decompose the nitric acid, and the affinity of the alkali begins to act; but at a white heat it is acted upon with great energy by that salt.

With carbonate of potash, silicium burns very easily with a vivid inflammation; carbonic oxide is given out, and the reduced carbon imparts a black colour to the mass. The incandescence is so much the more intense, and the temperature requires to be so much less elevated in order to determine the action, as less carbonate of potash or of soda is taken. Thus, for example, taking a portion of carbonate equal in bulk to half the silicium, the inflammation takes place much below the temperature of ignition. With greater quantities of the carbonate, the mass becomes distended by the production of the carbonic oxide, takes fire and burns with a blue flame. If a still greater quantity of carbonate be employed, no signs of combustion can be perceived; the mass does not grow black, and merely gives out carbonic oxide.

From this mode of action of silicium with carbonate of potash arises a very paradoxical phenomenon. If some incombustible silicium is heated with saltpetre to a moderate red, on a leaf of platinum or in a little crucible, no action will be observed; but if there is added a little dry carbonate of soda, so that it may touch the silicium, a detonation will take place, at the expense of the carbonate, in the midst of the saltpetre, and the mass will preserve for some time its black colour.

The cause from which the silicium burns at a low temperature more easily with the carbonate than with saltpetre, is without doubt owing to the affinity of the alkali for silica being necessary to determine the combustion of the silicium, and not being able to manifest itself with the saltpetre except when the temperature is sufficiently elevated for the decomposition of the nitric acid. If the burnt mass continues black some time longer, this arises from the new combination being compact, and protecting the carbon until it enters into fusion.

Silicium detonates with the hydrates of the fixed alkalies, producing a vivid incandescence at a temperature at which they melt, but much below that of ignition. Hydrogen gas is given out, which is seen to burn when the bulk of the mass is not too small. The same phenomena are observed with the hydrate of barytes. Incandescence also takes place with the hydrate of lime; but it is very feeble, and the silicium is oxidated but imperfectly. With the acid fluate of potash it detonates at the fusing temperature of the salt, that is to say, much below a red heat; it is not affected by fused borax.

Silicium heated to a perfect red in the vapour of sulphur inflames and burns, but with less intensity than in oxygen; and the combination even does not take place with the incombustible silicium. The sulphuration is usually as incomplete as the oxidation, and a scorified mass is obtained of a dark gray colour. It sometimes happens, however, particularly when a vacuum is made in the vessels before volatilizing the sulphur, that the silicium becomes completely sulphuretted, at least in a part of its mass. It then presents an earthy white body, which, thrown into water, instantly dissolves in it with the evolution of sulphuretted hydrogen. The silicium is converted into silica which dissolves in the water; and if this is in small quantity, a solution may be obtained so much concentrated, that it solidifies after a slight evaporation, and it leaves the silica, after the desiccation, in a transparent fissured mass. Silicium imperfectly sulphuretted also decomposes water rapidly, with the disengagement of hydrogen, and solution of the silica in the water; and the silicium which was not combined with the sulphur becomes separated. In the air the sulphuret of silicium diffuses a very strong smell of sulphuretted hydrogen, and loses in a little time all its sulphur; but in dry air it may be preserved a long time. At a red heat, it contracts and shrivels up, yielding sulphurous acid and silica. This change, however, takes place but slowly; for when kept at a red heat for some moments, it still has the property of decomposing water.

The siliciuret of potassium readily combines with sulphur at a red heat; but if the mass is dissolved in water, there remains much silicium, unless the mass be newly exposed to a white heat, because the silicium then combines with the sulphur at the expense of the potassium previously sulphuretted at a more elevated temperature. This combination is a true double sulphuret; its colour is of a dark brown, almost black. It presents a melted mass which dissolves in water. It is difficult to say whether it dissolves without changing its nature; but since the sulphuret of silicium is decomposed in water, and sulphuretted hydrogen has a great affinity for the sulphuret of potassium, it is very probable that in the solution there exists some silicate of potash with hydro-sulphuret of the same alkali. The combinations of the sulphuret of silicium with the metallic sulphurets, although they exist in the dry way, cannot, it appears, exist in water.

It is certainly a very remarkable fact to see the silica dissolve in so great quantity in water at the moment of its formation, and lose this property by evaporation, so as to become insoluble in acids. This great solubility, shown by the preceding experiments, explains the numerous crystallizations of silica in the cavities of druses, which sometimes could not contain a volume of liquid ever so little greater than the crystal itself. Nevertheless, I do not pretend that the silica had been in solution as in our example; that is to say, that it was the result of the decomposition of the sulphuret of silicium.

I did not succeed in combining phosphorus with silicium by bringing it into contact with the latter at a red heat. I did not try any other process.

If silicium is heated in a current of chlorine, it takes fire and continues to burn. If the gas contains atmospheric air, some silica remains in the form a slight skeleton. Silicium burns equally well in chlorine, whether or not it has lost its combustibility in the air. The product condenses and presents a yellowish liquid when it contains an excess of chlorine, but which is without colour when it is freed from this excess. This liquid is very fluid; it evaporates almost instantaneously when exposed to the air, yielding white vapours, and leaving a residuum of silica. It has a very penetrating smell, which may be compared in some degree to that of cyanogen. Thrown into water, it floats on the top; it generally dissolves in it, or leaves a little silica. If the quantity of the water is small, a drop, for example, on as much choride of silicium, this envelopes it, and the silica remains in a frothy semi-transparent mass. This liquid is analogous to the combinations of the other electro-negative bodies with chlorine. It reddens litmus paper; it is the second example known of a liquid combination formed by silicium.

At common temperatures potassium has no action on chloride of silicium; but when heated in the vapour of the latter substance it takes fire and produces a compound of potassium and of silicium. The iodide of potassium does not combine with silicium.

Silicium is neither dissolved nor attacked by the sulphuric, nitric, or muriatic acids, and not even by aqua regia. In its combustible state it is slowly dissolved by fluoric acid with the evolution of hydrogen; but in losing its combustibility, it loses also the property of dissolving in this acid. It is, on the contrary, dissolved with rapidity, even in the cold, by a mixture of the nitric and fluoric acids, nitrous gas being given out. Combustible silicium is dissolved by digestion in a solution of caustic potash; but when rendered incombustible, it is no longer attacked by the alkalies in the humid way.

Silicium, when once isolated, combines with the metals with much difficulty. Its remarkable affinity for platinum is known by the experiments of M. Boussingault; but it may be heated as often and as long as we choose in a platinum crucible, without any combination taking place. But if we endeavour to reduce silicium by potassium in a platinum crucible, the silicium penetrates deeply into the platinum where-ever this is touched by the potassium. Copper, silver, lead, and tin, heated with silicium by the blowpipe, do not seem changed in their appearance, nor in their ductility; notwithstanding when they are treated with acids they leave a small quantity of silica. The copper, particularly, left a skeleton of the same form as its own. It is here to be remarked that silicium, which alone is not attacked by the acids, is oxidated by them when its combinations with the metals are dissolved. We have, however, a similar example already in rhodium, which, though not attacked *per se* by aqua-regia, is dissolved by it when alloyed with certain metals.

Titanium, which approaches so near to silicium in its properties, is also insoluble in acids in the metallic state (with the exception of a mixture of the fluoric and nitric acids), whilst it is oxidated and easily dissolved when alloyed with other metals.

Silicium combines with potassium at a high temperature, but without the evolution of a remarkable heat. It affords two combinations; one, with excess of potassium, of a dark-gray, and which dissolves completely in water; the other, with less potassium, is obtained by the reduction before stated, or by exposing the first to a very strong heat. It is besides probable that silicium may form with the metals combinations, the proportions of which correspond with those of their silicàtes.—I reserve these researches for another time.

[*To be continued.*]

Art. V.—*On a Scarlet Sub-chromate of Lead, and its Application to Painting and Calico Printing.* By John Badams, Esq. [*Ann. Philos.*]

It was announced by Dulong, in the Ann. de Chim., so long ago as the year 1812, that by boiling together carbonate of lead and chromate of potash in excess, a red sub-chromate of lead is produced, containing exactly double the quantity of lead in the common chromate. Ten years afterwards, in the same journal, Grouvelle announced the existence of a red chromate, and gave several processes for preparing it, but made no mention of Dulong's discovery. It is remarkable that neither of these chemists, nor any subsequent writer, appears to have noticed the important uses to which this substance is applicable.

Having made a variety of experiments upon its properties, as a fast colour in calico-printing, and a durable pigment for artists in oil and water, I wish to give publicity to the advantages which, I think, the arts will derive from its adoption.

Grouvelle's method of preparing red chromate of lead consists in boiling the common yellow chromate with potash; for practical purposes, this is much more convenient than Dulong's process, and gives a very fine colour. By some unaccountable blunder, however, the ingenious author of this formula so completely mistakes the nature of the substance produced by it, that every step he takes in his analysis to explain the composition of the red chromate, only tends to involve it in deeper obscurity. But he must speak for himself.

"J'ai analysé comparativement le chromate jaune, le rouge, et le plomb rouge de Sibérie. Tous donnent *exactement* le même rapport entre *l'acide* et *l'oxide*. Ce sont des chromates neutres; seulement le chromate rouge contient une petite qauntité d'alcali, qui m'a paru etre de là 1. 5;" and yet in the very next page, the author contradicts himself, and says, in summing up,

"Il suit de là que l'alcali parait être combiné à l'oxide de plomb, et que cette combinaison unie au chromate de plomb, donne lieu au chromate rouge, qui contient ainsi un peu *plus d'oxide* de plomb que le chromate neutre."

Grouvelle then goes on to assert, that he finds little alkali (lime) in the native red lead of Siberia, though, as he admits, this very ore becomes yellow on being powdered, which, if it really contained lime, it ought not to do.

I shall now endeavour to show that the whole of this analysis is erroneous.

(A.) 100 grains of scarlet chromate of lead made by Grouvelle's process, were digested for half an hour with constant stirring in very diluted acetic acid. The red colour gradually but totally disappeared, and was succeeded by a pure yellow. This yellow powder, carefully washed and dried at 300°, weighed 60 grains, and proved on examination to be common chromate of lead.

Here then we have red chromate of lead (said by Grouvelle to contain exactly the same proportions of oxide and acid as the yellow chromate) resolved into 60 grains of yellow chromate and 40 grains of some other substance, which we shall see, by the next experiment, to be oxide of load.

(B.) The acetic acid, holding in solution 40 grains of the original weight of the red chromate employed, was perfectly colourless and transparent; and on being gently evaporated in a water bath, shot entirely into crystals, bearing the well-known character of acetate of lead. On redissolving these crystals in water, and boiling them with an excess of carbonate of ammonia, a dense white precipitate fell, weighing, when dried, 46 grains=38.45 nearly oxide of lead.*

(C.) To ascertain whether any sensible portion of potash originally entered into the composition of this red chromate, the ammoniacal solution was evaporated gently in a large platina crucible, and gradually exposed to a red heat; the whole residue sublimed, leaving only a scarcely perceptible stain of lead upon the surface of the crucible. A little water now boiled in the same crucible did not affect the colour of turmeric paper in the slightest degree; hence no potash whatever can be suspected.

On repeating these experiments, I feel satisfied that the 40 grains of loss sustained by the red chromate in experiment (A), were entirely due to oxide of lead; and that 100 grains of red chromate were resolved into 60 grains of yellow chromate and 40 oxide of lead. But 60 grains of yellow chromate are composed of 40.98 oxide of lead, and 19.02 chromic acid; hence it cannot be doubted that yellow chromate contains just half the proportion of oxide in the red chromate; in which case the atomic constitution of the two chromates of lead will be as follows:

Yellow Chromate.	*Red Chromate.*
Chromic acid....19.02, 1 atom	Chromic acid...19.02, 1 atom
Oxide of lead....40.98, 1 atom	Oxide of lead...81.96, 2 atoms
Or 100 parts consist of	
Chromic acid..................31.7	Chromic acid...............18.84
Oxide of lead..................68.3	Oxide of lead...............81.16
100.00	100.00

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[*] The difference between 38.45 and 40 is owing to the imperfect precipitation of oxide of lead by carbonate of ammonia.

In order further to show the true composition of red sub-chromate of lead, as I may now venture to call it, I took 60 grs of yellow chromate and ground them with 40 grains of oxide of lead, adding small quantities of hot water from time to time: they united into a red sub-chromate.

Uses.

I have endeavoured, at some length, to clear up the theory of the two chromates of lead, because one radical error of the nature which Grouvelle has committed, leads to endless mistakes and disappointments in the practical operations which are founded upon it. I have before alluded to the advantageous use of sub-chromate of lead in giving a permanent orange scarlet upon cotton. The mode of its application to calico-printing will be obvious to all who know the principle upon which yellow chromate of lead is made fast; and no intelligent calico-printer will need any further information from me, as his own practice in fixing the yellow chromate, added to what has been here said of the nature and preparation of the red sub-chromate, will readily suggest what is necessary. I shall only remark that nitrate of lead and an alkaline solution of chromate of potash, will give him the colour. He may accumulate, besides, some insoluble salts of lead in the pores of the cloth, to give stability to the tint, and may modify the operation by various means, but in all cases the colours must be heightened at last, by passing them through boiling water.

Scarlet sub-chromate of lead is extremely beautiful when ground up with oil, and possesses great *body* as a pigment. It is not degraded in its hue, like vermillion, by admixture with white lead; it mingles with other colours, and shows no signs, after a long exposure, of any change by time. As a water-colour, I dare not yet say that it has been tried sufficiently to authorize a positive declaration that it will not blacken, but several pieces of cards and thin paper painted with it, and hung upon the walls of inhabited houses, likely to influence the colour of salts of lead, have not, in some months, perceptibly diminished in brightness. Should a longer experience confirm the promise already given, I shall be happy to make it known to artists, as they have nothing equal in colour to red lead which they can trust in their drawings for a month even, and no tint would be a more desirable accession to the pallet, than a bright and permanent scarlet, or scarlet orange.

Art. VII.—*Essai géognostique sur le gisement des Róchés dans les deux Hémisphères, par A. de Humboldt.* 1 vol. 8vo. Paris, 1823.——*A Geognostical Essay on the Superposition of Rocks in both Hemispheres.* Translated from the Original French. London, 1823.[*Ju .. Roy. Inst.*]

It is known that Baron Alexander Humboldt is a great traveller, and it is believed that he is a great Astronomer, a great Botanist, a great Political Economist, and to sum up all, a great Philosopher. It remained to be proved that he was also the great Geologist that he had been believed; and the proof is, the book before us.

The Title, implicating the whole sphere, (which is obviously produced by the addition together of two hemispheres,) would have led us to expect the history of all the rocks of our sphere, or globe, had we not been accustomed to the grandiloquism of our author; and we ought, therefore, to inform our readers, that the "gisements" in question are confined to a very small portion of Europe, and a much smaller one of America. The other two hemispheres, Asia and Africa, will probably appear in a future volume. But this work embraces "pour ainsi dire, toute la geognosie positive;" a fact which we are very happy to hear, as the shelves of the Geological Society may now discard the lumber, German, French, and Italian, besides English, with which they are loaded, and replace it with this volume of quintessence.

Before proceeding further, however, we ought probably to apologize to Baron Humboldt, to our readers, and to what is commonly called the world, for daring to take liberties with a Great Man, or a Great Name rather. It is the courtesy of society to fall down and worship the idol which it has set up, and thus that Scotch Entity, called the Great Unknown, is permitted to write with impunity even *St Ronan's Well.* All that we can say, however, in our defence is, that whatever respect we may have for Trojan or Tyrian, we have more for truth; and that if Plato himself had written on the two hemispheres, we should have equally asked, what he knew of them.

Why will not M. Humboldt tell a plain tale plainly; if he had useful geological information to give on an unknown country, he might have done it without all this *pretension.* His book is not a system of Geognosy, and it is not a description of the positions of the rocks in the two hemispheres. The Equator, the Andes, the Amazon, the Oroonoco, are sonorous words, but the world already knows, and is wearied of hearing, that he has travelled them; weary of his travels, weary of his latitudes, weary of his isothermal lines, weary of his geography of plants and animals, and weary of him. The very Faubourg St Germain, the saloons, the coteries, are as weary of the Andes, as are Cuvier, Arago, and Fourrier. We must not, however, transgress the circle here drawn round us; yet, in this narrow book, we can find matter for caution and advice, which in sincerity and friendship, whatever M. Humboldt may think, we offer to him. He has seen much that others have not; he has collected facts that others have not; he has travelled, and he might have instructed; but not content with such fame, and such praise as he might have commanded, he has been resolved to be a philosopher and a Sublime; to Chateaubriandize on weeds, and algebraize on stones, forgetting that he who has placed one foot on the pinnacle is thus much nearer to the bottom—"il n'y a qu'un pas."

There is a certain singleness and simplicity of apprehension necessary to writing clearly, and this is a quality, we grieve to say, which M. Humboldt does not possess. If there be an idea, it is involved in such a mass of words that it is suffocated; if there be two, they become so entangled that their separate identity disappears. The Baron's writing appears to have set for its portrait to M. Fuselis' lectures on painting. The purpose of language, says Swift, (among others) is to conceal your thoughts; but it is another important purpose, which he has overlooked, to be a substitute for them.

There are always two terms, a good and a bad one, for every human action, for every thing. Michael Angelo is grand, or extravagant and absurd. M. Humboldt professes to generalize; his views are grand, broad, poetical; or else they are fictitious, slovenly, or dreamy. The mob has voted "the ayes;" we are of the contrary opinion. In philosophy, in science, natural or otherwise, we know of no generalization that is not a collation and abstract of facts. Without this, it is all guess and fancy; and the fancies are commonly abundantly dull, moreover. Isothermal curves may captivate the uninquiring herd, as do the crotchets of algebra, since they look profound, like lord Burleigh; but like lord Burleigh, it is to say nothing. We should be much more grateful to M. Humboldt if he would particularize; we do not want pages of guessing; and if we are to read poetry, let it be John Gilpin or Childe Harold; nor "Goddess write about it and about it."

Can we now convince M. Humboldt that we are his friends and well wishers? if we tell him of his faults, where else will he find sincerer ones? But we must proceed to the "Gisement des Rôches."

Upwards of 80 pages are occupied in something that looks like an introduction, being a loose collection of facts, words, and opinions; and this is followed by an account of all the acknowledged rocks, from granite upwards. Such is the plan of the work, which is nearly as undigested as its materials are indigestible.

To give any thing like an analysis of the introductory matter, which seems intended as a sort of abstract of the science, is impossible. We are lost amid the vagueness of what might have been compressed into half the space, or less, while it ought to have been divided into paragraphs or subjects. It is not thus that philosophy is to be written or taught; and if this be Geognosy, we shall remain content with simple Geology. If there is a want of precision, of *definiteness* in the manner, there is not less in the matter; while with a mixture of pedantry and modernism, there is that antiquated phraseology, derived from Freyberg, which shows that the author has not reached up to the level of our present knowledge, but is merely ingrafting a few facts from a better source, on the cant and dogmas of his school. The want of purpose, the vagueness of ideas, the parade of science, which are so characteristic of this writer, will perhaps be rendered as visible by the following passage as by any other; and it is really selected without any invidious motives, and as an example of his style and turn of thinking

"We must not confound (a point on which I have often insisted in this essay) rocks passing insensibly to those with which they are in immediate contact; for instance, mica-slates that *oscillate* between gneiss and clay-slate, with rocks that alternate together, and preserve all their distinct characters of composition and structure. M. d'Aubuisson has long since shown, that chemical analysis connects clay-slate with mica. (*Journal de Physique*, tom. lxviii. page 128. *Traité de Géognosie*, tom. ii. page 97.) The former it is true has not, indeed, the metallic lustre of mica-slate: it contains a little less potash and more carbon; the silica does not unite in knots or thin plates of quartz, as in mica-slate; but there can be no doubt that the scales of mica constitute the principal basis of clay-slate. These scales are so united together, that the eye cannot distinguish them in the mass. Perhaps this very affinity prevents the alternation of clay-slate and mica-slate; for in these alternations nature seems to favour the association of heterogeneous rocks, or, to use a figurative expression, she delights in those associations in which alternating rocks exhibit a great contrast of crystallization, mixture, and colour. I saw in New Spain dark-coloured greenstone alternating thousands of times with reddish-white syenites, that abound more in quartz than in feldspar; veins of syenite occur in this greenstone, and veins of greenstone in the syenite; but neither of those rocks passes into the other. (*Essai Politique sur la Nouvelle Espagne*, tom. ii. p. 523.) At their actual contact, they exhibit differences as strongly marked as the porphyries that alternate with grauwackès and syenites, the black limestone that alternates with transition clay-slate, and many other rocks altogether heterogeneous in their composition and general aspect. It may be observed further, that when in primitive formations, rocks more nearly allied to each other from their composition than by their structure, or their mode of aggregation, for instance, granite and gneiss, or gneiss and mica-slate, alternate, those rocks do not show the same tendency to pass into each other, which they display in non-complex formations. We have already observed above, that a bed β, when it occurs more frequently in the rock α, points out to the geognostic traveller, that the simple formation α is about to be succeeded by a complex formation, in which α and β alternate. Afterwards it may happen that β assumes a greater developement; that α is no longer an alternating rock, but merely a subordinate bed to β, and that this rock β appears by itself, till by the frequent appearance of beds γ, it forms the prelude to a complex formation of β alternating with γ. We may substitute to these signs the words granite, gneiss, and mica-slate; those of porphyry, grauwackè, and syenite; of gypsum, marl, and fetid limestone (*stinkstein*); but the *pasigraphic* language has the advantage of generalizing problems, and conforms more to the requisites of *geognostic philosophy*, of which I here attempt to trace the first elements, as far as regards the study of the superposition of rocks. But, between formations, simple and nearly connected in the order of their relative antiquity, between the formations $\alpha\,\beta\,\gamma$, complex formations, $\alpha\,\beta$ and $\beta\,\gamma$, are often found placed, (that is, α alternating with β, and β alternating with γ;) it is also observed, though less frequently, that one of those formations (for instance, α) takes an increase so extraordinary, that it envelopes the formation β, and that β, instead of appearing as an independent rock, placed between α and γ, is only a bed in α. Thus, in Lower Silesia, the red sandstone contains the formation of zechstein; the limestone of Kunzerdorf with impressions of fish, and analogous to the bituminous marl of Thuringia abounding also in fish is entirely enveloped in the coal sandstone. (Buch. *Beob.* t. i. p. 104, 157; Id. *Reisenach Norwegan*, t. i. p.

158; Raumer, *Gebirge von Nieder-Schlesien*, p. 79.) M. Beudant (See *Min.* t. iii. p. 183,) has observed a similar phenomenon in Hungary. In other regions, for instance, in Switzerland, and at the southern extremity of Saxony, the red sandstone disappears altogether; being replaced, or in some sort suppressed, by an immense developement of grauwackè, or of alpine limestone. (Freiesleben, *Kupfersch*, b. iv. 109.) These effects of the alternation and unequal developement of rocks are so much the more worthy of attention, as the study of them may throw light on some apparent deviations from a type of superposition generally received, and may serve to refer to a common type the series of positions observed in countries very distant from each other.

We might make many commentaries on this extract, but shall content ourselves with few. We do not see any necessity for "insisting" on our not confounding transitions of rocks into each other with mere alternations; and cannot perceive the vast value of the caution, nor the importance of the enunciation, as if it were the author's own discovery. As to M. d'Aubuisson, with his mica and thonschiefer, he is worthy of Humboldt. We may ask either, or both, what on earth the chemical analysis of a mixed rock proves, and what important consequences follow from knowing that mica and clay-slate contain the same earths. The analysis of a hundred sandstones would correspond with that of as many granites; hornblende contains the same earths as clay-slate, and in the same proportion; so does shale, so does clay itself, so does basalt, so does greenstone, and so does pitchstone. We may add, with Voltaire's "Nonchalant," what then? As to our "not doubting that scales of mica constitute the principal basis of clay-slate," we certainly not only doubt, but deny it. The one is a crystal, or a crystallized mineral, the other not; and if we were to seek for an identity "not to be doubted," it would rather be between clay and clay-slate.

That nature "favours the association of heterogeneous rocks," is one of those frivolous and false generalizations which characterize the whole of this author's writings. It is not the fact, and if it were, what then? If there be great spaces or depths of one rock without intermixture, why then there are; and if there are not, why then nature must "please herself" with differences. These are words; and thus it is with this author, as if a different wordage was philosophy and a discovery, a generalization and *une belle idée*.

There is another of these general laws equally unmeaning, and, to say the truth, not exactly intelligible, at least in English, whatever it may be in French or German geognosy, or in the geognosy of New Spain. It is that "when in the primitive class, rocks more nearly allied to each other from their composition than by their structure or mode of aggregation, such as granite and gneiss, or gneiss and mica-slate, alternate;" "those rocks do not show the same tendency to pass into each other, which they display in non-complex formations." It is not by discovering such laws as this that geognosy or any gnosy will ever become a science.

Here also we stumble on the threshold of the author's new geognostic algebra; and unquestionably, there is much profundity in a pasigraphy of alpha, beta, gamma, delta; but as we find that this new lithological algorithm is lying in wait for us again in the appendix, we shall for the present pass it over, as well as the remainder of a paragraph, which, thus far, we owed to our readers.

There are many ways of writing a new book, or a new paragraph, or of seeming to have discovered a new idea, or to have made a discovery; and there is none much more effective than that of translating a plain, vulgar, intelligible, well-known fact or proposition, into long-winded, altisonant, and obscure phraseology. For example, it does not follow that, because any given limestones, such as that which lies beneath the red marl, contains pectines or terebratulæ in England or Greenland, it must also contain them in Peru or Napaul, and it is not true that because an echinus belongs to chalk, it must not also exist in slate. Nothing can be plainer; the truth is, that it is a great deal too plain. Now mark how it is improved by dress, how it is adornized, till we are as puzzled to recognise it as we should be to know Mr Waithman, when tricked up in all the robings and state of a lord Mayor.

> "In the case of the conformity of position, there may be identity of mass (that is, of mineralogical composition,) and of diversity of the fossils or diversity of mass and identity of fossils. The rocks β and β, placed at great horizontal distances, between two identical formations, α and γ, belong either to the same formation, or are parallel formations. In the first case, their composition is similar; but on account of the distance of places, and effects of climate, the organic remains which they contain may differ considerably. In the second case the mineralogical composition is different, but the organic remains may be analogous. I consider the words *identical formations, parallel formations*, as indicating the conformity or non-conformity of mineralogical composition; but they do not enable us to prejudge respecting the identity of fossils. If it be sufficiently

probable that the deposits β and β, placed at great horizontal distances between the same rocks, α and γ, are formed at the same epochs, because they contain the same fossils, and are of the same composition, it is not equally probable that the *epochas of formation* are very distant from each other, when the fossils are distinct. It may be conceived that in the same zone, in a country of small extent, generations of animals have succeeded each other, and have characterized, as it were by particular types, the *epochs* of formations; but at great horizontal distances, beings of various forms may have occupied simultaneously, in different climates, the surface of the globe, or the basin of the sea. It may be observed further, that the position of β between α and γ proves that the formation of β is anterior to that of γ, and posterior to that of α; but nothing gives us an absolute measure of the intervals of time (*les époques-limites*); and different insulated deposits of β may not be simultaneous."

The man who, like Mr Hazlitt, proposes to cover a certain surface of paper with a certain number of words, would be very silly if he were to trouble himself about ideas, propositions, or arguments. The public expects nothing but a certain jack-o'-lantern flitting and dancing, which amuses the eye without encumbring the thinking powers; and it is satisfied. It reads, because it is necessary or convenient to read; and the repose of its faculties remains undisturbed. And this is very charming reading too; as they will vouch, who have scanned, with never exhausted delight, the exquisite verses of a "Person of Quality," which the prophetic mind of Pope sent forth, a model to succeeding ages. But he who, like Eudoxus or Euclid, deigns to assert some fact, or to give reasons why a fact should be believed, or to do both the one and the other, has generally been desired to tell the nature of his proposition and to assign his reasons. The very tailor himself, is indeed unreasonable enough to expect something of this kind, when he is to make a coat, and when it is to be, or not to be, made with a Brummel collar, or a Petersham pocket. In all the sciences, from piquet to political economy, as Hoyle and Adam Smith have shewn, and even in geology or geognosy, those who either understand, or desire that others should understand, usually state propositions and subjoin reasons; and, most commonly, when a man writes a paragraph, it is a paragraph, and nothing else. It contains a head, a body, and a tail, much in the manner of a horse, or of one of Cicero's orations; and does not run out into protuberances, or comprise various heads and tails, or, beginning with the history of a cock, terminate with that of a bull.

The figure called rigmarole has not generally been held in very particular esteem in the mathematical sciences. It is lucky, however, that it is approved of by the geognosts; and really there is such an affinity, and a harmony, between the two categories of geognosy and rigmarole, that we begin to think ourselves in error in doubting the utility and adaptation of the manner before us to the matter. We were about to have said, "De grace, Monsieur le Baron, humanisez votre discours et parlez pour être êntendu." But to what purpose; when there is nothing to be understood?

We have heard of a man running away with an idea, and we have also heard of an idea running away with a man. We have no great objection to either. But when a man is "run away with" by half a dozen at a time, his sentences and paragraphs are apt to become as disjointed as he himself would be, had he been run away with by as many wild horses. There is a vulgar maxim, called sticking to the point, which has been sometimes recommended, and which might be advantageously applied somewhat more widely, as for example:—

> "From these general considerations on zoologic characters, and the study of fossil bodies, it follows, that notwithstanding the admirable labours of Camper, Blumenbach, and Sömmering, the exact determination of species, and the examination of their relations with beds very recent and close to the chalk, does not date farther back than twenty-five years. I believe that the study of fossil organic bodies, applied to all other secondary and intermediate beds by geognosts, who consult at the same time the position and mineral composition of rocks, far from overthrowing the whole system of formations already established, will rather serve to support, improve, and complete its vast series. The geognostic science of formations may, no doubt, be investigated (*envisager*,) under very different points of view, according as we give a preference to the superposition of mineral masses, to their composition (that is, their chemical and mechanical analysis,) or to the fossils which are contained in many of those masses; the whole of these are included in the science. The denominations *geognosy of position*, or of *superposition*, *oryctognostic geognosy* (which considers the texture of the masses), and *geognosy of fossils*, designate, not branches of the same science, but various classes of relations which it is necessary to insulate, in order to study them more particularly. This unity of the science, and the vast field it comprehends, were well recognised by Werner, the founder of positive geognosy. Although he did not possess the necessary means for attaining a vigorous determination of fossil

species, he never failed, in his course of lectures, to fix the attention of his pupils on the relations that exist between certain fossils and formations of different ages. I witnessed the high satisfaction which he felt, when M. de Schlottheim, one of the most distinguished geognosts of the school of Freiberg, began in 1792 to make those relations the principal object of his studies. Positive geognosy has been enriched by all the discoveries that have been made on the mineral constitution of the globe, and furnishes valuable materials to another science, im properly called the *theory of the earth*, which comprehends the first history of the catastrophes of our planet. It reflects more light on that science than it receives in its turn; and without contesting the ancient fluidity or the softness of the stony beds, (a phenomenon proved by the fossil bodies, by the crystalline aspect of the masses, by the rolled pebbles, or the fragments imbedded in the transition and secondary rocks,) positive geognosy does not pronounce on the nature of the liquids in which it is said that the deposits were formed, those *waters of granite, porphyry*, and *gypsum*, which in hypothetic geology, are made to arrive tide by tide on the same point of the globe.

For one paragraph, this is somewhat lengthy, as Jonathan might say. What the proposition may be, is another question. Is it fossil remains, or a method how to "envisager" geognosy, or to "envisager" the merits of Werner, or about a theory of the earth which is not a theory, or to prove that though Werner is the greatest geognost that ever lived, he had no business to make "those *waters of granite, porphyry*, and *gypsum*," arrive, "tide by tide sur un même point du globe?" When a man sits down to write a paragraph, he would find it convenient to ascertain what he means to say. He should take a long breath, also, lest he run himself out of wind; an accident which we have observed to happen, in conversation, to those who deal in these same interminable paragraphs.

As to what may be the main matter, or rather one of the main matters of the aforesaid paragraph, we really have some doubts respecting the creative powers of Werner in positive geognosy. The positive quality we grant, as this geognosy never betrayed any want of confidence and assertion. The remainder might be mooted if we were much inclined that way. We remember the late Mr Tennant, and we remember his pronouncing in one of his lectures respecting this creator of positive geognosy,—"a stupid German blockhead;"—"aside," however, as is sometimes the usage of the stage. Really, Mr Tennant, these were hard words;

but Mr Tennant had not scaled Cotopaxi or Popocatepetl.—Dans les pays des aveugles les borgnes sont Rois.

There is a vast virtue in hard words, just as there is in the *x*'s and *y*'s of Algebra. Baron Humboldt is not unacquainted with the value of "Loxodromisme," however we may be. The office near Soho-square for plundering, alternately, servants and masters, would lose half its attractions but for its Therapolegia inscription. The guinea paid for supplying it was well bestowed. Loxodromism or Loxodromy. What a mass of knowledge is contained in this little word. Elevation, parallel position, these are household terms, and carry no weight. The clown does not admire the preacher whom he understands; "he is no Latiner." When a man can express himself in the ordinary language which he has picked up from his nurse, we have generally a suspicion that he knows what he means. Solomon seemed to be of the same opinion, when he complained of the man who "darkened counsel by words without knowledge."

This Greek coinage is a false money, which cheats us with a semblance for solid gold. We wish the French nation would burn its Lexicons, since it has not yet learnt to make any other use of them. Great evils arise from petty causes, now and then. Had Brongniart not been the lucky (unlucky for us) possessor of a Greek lexicon, we should have been spared Phyllade, and Dolerite, and Trachyte, and Psammite, and Euphotide, and Diabase, and heaven knows what more; we should have been spared the arrangements, and the system, and the whole, and we might now have understood what his nation is writing about, if indeed they understood it themselves.

But the Greek is not the sole criminal, for the German does as well. Geognosy, or aught else, there is nothing so easy as phrases, phraseology, conventional language; cant, we were very nearly saying. All the Gnosies have their phrases and their cant; and words go for sense. They save the trouble of thinking, which is a vast convenience; and of understanding, which is more convenient still. Explain, explain, as they say in the House of Commons. Read Swift, and learn to write your own language; read Blaise Pascal, Mons. le Baron.

But we must return to the Loxodromy, the oblique course of the strata; and of Mr Humboldt's ideas. And now we shall see the reasons which induced the **Great Traveller** to travel to Pichinca and Portocabello.

"Since the year 1792 I have been attentive to this parallelism, or rather to this *loxodromism* of beds. Residing on mountains of stratified rocks, where this phenomenon is constant, examining the direction and dip of primitive and transition beds, from the coast of Genoa across the chain of the Bochetta, the plains of Lombardy, the Alps of St Gothard, the table-land of Swabia, the mountains of Baireuth, and the plains of Northern Germany, I have been struck, if not with the constancy, at least with the extreme frequency of the directions nor. 3-4ths of the compass of Freiberg (from south-west to north-east). This inquiry, which I thought would lead naturalists to the discovery of a great law of nature, at that time interested me so much, that it became one of the principal reasons for my voyage to the equator. When I arrived on the coast of Venezuela, and passed over the lofty littoral chain and the mountains of granite-gneiss that stretch from the Lower Oroonoko to the basin of the Rio Negro and the Amazon, I recognised again the most surprising parallelism in the direction of beds; that direction was still nor. 3-4ths (or N. 45° E.), perhaps, because the littoral chain of Venezuela is not far from the angle which the central chain of Europe forms with the meridian. I announced the first results of observations which I made on the primitive and transition rocks of South America, in a Memoir published by M. Lamétherie, in his *Journal de Physique*, tom. liv. p. 46. I had mixed (as is often the case with travellers, who publish the results of their labours during the course of their journeys,) some very precise observations on the great uniformity in the direction of beds, (at the Isthmus of Araya, the Silla of Caracas, at Cambury near Portocabello, on the banks of the Cassiquiare: see my *Relat. Hist*, tom. i. p. 393. 542. 564. 578., tom. ii. p. 81. 99. 125. 141.) with some general views which I have since regarded as vague and less accurate. Four years of travelling in the Cordilleras rectified my ideas on a phenomenon far more important than was hitherto believed; and at my return to Europe, I hastened to publish the general result of my observations in the *Geographie des Plantes*, p. 116.; and in the *Essai Politique sur la Nouvelle Espagne*, tom. ii. 520. Those results remained, no doubt, unknown to the learned author of the *Critical Examination of Geology*, p. 276., where he combatted the assertions published during my absence, in 1799, by M. de Lamétherie."

Now does Mr Humboldt really conceive and assert that he was the discoverer of the continuous and orderly elevations of strata, that this was an object to run away to the equator about, and need he tell us, for the twentieth time, that he is *The Traveller* of New Spain, that he has published a Geographie des Plantes, an Essai Politique sur la Nouvelle Espagne, a Relation Historique, and a book of Personal Narrative? As to the "importance" of the fact, he surely does not imagine that it was pointed out by him; and as to his ideas having been rectified by four years of running about the Cordilleras, we only grieve that he did not continue his "courses" for forty, that they might have undergone a general rectification.

We have no dislike to M. Humboldt. On the contrary, we esteem him as an industrious and amiable man, ambitious, busy, and good-humoured, really, qualified, in many very important matters, for a general traveller. But his ambition has been of too vaulting a nature, and he has overleaped the point of success and security. He would write of every thing, and on every thing, he has been vague, inaccurate, wordy, and wearisome. He would be the name to fill all Europe with its sound, and he has, for this purpose, filled it with his books. He has blown the trumpet for himself, and he has found friends to blow it for him. There is no base for all this building. It is too much the custom of the day to work up to some niche in the temple of Fame, by contraband procedure, and it is not for M. Humboldt's sake alone that we make these remarks

Public justice demands that all should have equal justice. Of fame, there can never be but a certain portion in the world; for, where all are rich, no one is wealthy. If it is to be monopolized, let it be alloted where it is merited, for, when it is alloted otherwise, the meritorious must be robbed of their share and their rights. The Journalist and Reviewer is the guardian of public justice in Literature and Science; and it is his duty, if often neglected, to see that praise is duly and justly alloted, that he may thus protect the feeble, or the neglected, whom the public will not protect, and who cannot protect themselves. That public will not listen to him, or about him, who is not a Humboldt (as the phrase is,) or a something else; but it never stays to inquire what a Humboldt is, or what a Humboldt has really done. For the sake of the injured and the oppressed, for the sake of civil justice, we must strip off all that is fallacious or borrowed; we might do it for the purpose of criminal justice, as is the fashion with modern criticism, that we might punish him who deserves punishment. But this is not our object; and, from this intention, we desire that the subject of our remarks will exempt us.

We are not criticising the total works of this author, but we may say that he who has not simply been ambitious to shine alike as a general traveller, as an astronomer, a botanist, a geologist, a politician, and much more, but who has laboured, directly and indirectly, to make the world believe all this, and who has contrived to succeed, ought to have been better informed, more accurate, and more dependant on his own powers and resources. Had he been all this, indeed, we should have heard far less of him, for he would have been more modest, and modesty is not now the purchase money of fame. There may be an excuse for bad writing, and confusion of thought and repetition, because an author cannot go beyond his powers. But there is little excuse for the perpetual ambition of being every thing; and there is none for inaccuracy, where accuracy is asserted, and where it is easily attained, nor for claims to originality founded on the labours of others. He at least who carries quadrants and wearies us with angles and immersions, is bound to be correct in longitudes, and most certainly in latitudes; and he who publishes the statistics of New Spain, would have judged more wisely had he informed us that they were a collection of public documents, and not the proper result of his own labour and observation.

But we must pass on to the work before us, and to the geology, or geognosy of M. Humboldt. What we have extracted, will serve to show the laxity and vagueness of this author's ideas, no less than his writings. And it is not for the poor purpose of criticising him alone, that we have examined his book; but because, through him, we hope to induce other writers on the same subject, to introduce more precision and purpose into their geognostical writings. We are already encumbered with a mass of useless and unintelligible matter, fit only for bonfires, and if we are to go on thus, a library of geology will soon rival the libraries of physicians. This is an age in which every man can write something, and in which every man thinks it necessary to write a book on something. Geognosy has the especial merit of being an easy subject, "heaven bless the mark," since by means of Hungary, Saxony, flötz, transition, and dolerite, with a little aid from loxodromism, hyenas, potamoid, acephalous, types, and a few more magic words, volumes are made, as volumes have been made before. Some mercy is due to the unhappy student, at least; and he who has thus seen the "high authority" of Humboldt doubted, may perhaps learn to doubt of other authorities, and perhaps also learn to fear lest the same measure may not be meted to himself, by future reviewers less humane than ourselves.

Those who please to consider M. Humboldt as very great in any other science, must enjoy their belief, as that is their affair, not ours. But it is our affair, to say that we do not consider him a great geognost, practically, and that we do consider him a very useless writer, not to say more. His book is meant, apparently, as it is declared to be, a system of geology, or geognosy, or a statement of all the facts of the science that have been ascertained; or a compendium, or detail, of "la geognosie positive." Assuredly, it is not that, in any sense. Of the introductory view, from which we have thus extracted, we can say nothing good or commendable. We have not selected passages to condemn, because they were faulty paragraphs, and, by so doing, attempted to misrepresent the whole. There is no plan nor purpose, and no information, in this long preface, which, in the hands of science and knowledge, might have easily contained what it pretends to do; a general view of the state of geognosy, and of its principal facts and relations. It is a sort of talk very much like the living conversation of the man himself; and we rise from the perusal without well knowing what we have been reading.

We have now, perhaps, said more than enough respecting this preface, yet we cannot part from it without recurring to a subject on which we touched before, and to a passage where the author becomes the critic and *laudator* of the Professor of Freiberg, of whose reign we really think it quite time that we should be weary. It is better to quote the passage than to extract from it.

> "Werner, in creating geognostic science, has perceived with an admirable sagacity all the relations under which we should view the independence of the primitive, transition, and secondary formations. He has shown what we ought to observe,—what it is important to know; he has prepared, and foreseen in some degree, a part of the discoveries with which, through him, geognosy has been enriched in countries which he could not visit. As formations do not follow the variations of latitude and climate, and phenomena, observed perhaps for the first time in the Himalaya, or the Andes, are found again, and often with an association of circumstances that seem to be entirely accidental, in Germany, Scotland, or the Pyrenees, a very small portion of the globe, a territory of some square leagues in which nature has assembled many formations, may, (like a true *microcosm* of the ancient philosophers), give rise, in the mind of an excellent observer, to very accurate ideas on the fundamental truths of geognosy. In fact, the first views of Werner, even those which that illustrious man had formed before the year 1790, possessed

a justness that is still remarkable. The learned of every country, even those who show no predilection for the school of Freiberg, have preserved them as the basis of geognostic classifications; and yet what was known, however, in 1790, of primitive, transition, and secondary formations, was founded almost entirely on Thuringia, on the metalliferous mountains of Saxony, and those of the Harz, on an extent of country not 75 leagues in length. The memorable labours of Dolomieu, and Saussure's descriptions of the Alps, were consulted, but these could not have influenced in a great degree the labours of Werner."

We suppress the remainder of this long paragraph, as less important. What we have quoted is sufficient to show that the critic is worthy of the subject, and each of the other. It will also tend to justify our own criticisms; for it is perfectly impossible, that any man really acquainted with geognosy, with the facts known, the history of the science, or its present state, could have formed such a judgment, or written such a paragraph.

Werner did not create "la science geognostique." He did little, and of that little, nearly all has proved to be wrong. There is not one of his general laws that has not been found utterly false; and if he ever became possessed of any facts, he proved that he was incapable of reasoning from them. The whole mass, nearly, of false induction and bad reasoning to be found in this science, is to be traced, directly and indirectly, to Werner and to Freiberg. It was, perhaps, a minor crime, that he taught what was untrue or useless, when he was the efficient cause of the ignorance and bad reasoning of a whole army of followers and admirers, and is still the night-mare of the science. If we have begun to shake off his paralyzing influence in England, it is far otherwise in France and Germany; and we, as far as we can, will not allow M. Humboldt to go on blowing the trumpet, because he himself has no other ideas of geognosy than those which he derived at and from Freiberg. We have as little animosity against Werner, or his ghost, as we have against M. Humboldt personally or impersonally; but we will maintain, that to perpetuate the praises of him whose dogmas are the impediment of a rising science, whose assumed infallibility is the stumbling-block of students, is a crime that demands and deserves reprobation, because it is adding to the weight and immobility of the great obstacle to our progress in that science.

It would require a whole essay like this, instead of a paragraph or two, to examine the details of Werner's demerits in geognosy, and to prove him in category, wrong. We cannot undertake to do it now, and moreover it is a disgusting and dull office. But what right has M. Humboldt to say, that the observations of Dolomicu "could not excercise any great influence over the labours of Werner?" If he meant it in irony, it would be true enough; since that dull and obstinate man shewed, through his whole life, that he was as incapable of making use of any one fact, and as unamenable to reasoning, as he was ignorant of all the necessary knowledge which was indispensable to the office he had undertaken. Wrapt up in his own unintelligible and impossible theories, he was satisfied with reigning a demi-god or a pope among the gaping herd that surrounded him, swallowing all his dogmas and doctrines, and repaying him with adulation and with volumes of his own "crambe recocta." Dolomieu might have taught him, if he had possessed sense enough to listen, that obsidian and pumice were not formed by water, nor volcanoes ignited by coal; in his own day, he might have learnt something of real geognosy from others than Dolomieu, if he had possessed talents of modesty enough to have been a pupil of those whom he professed to teach.

M. Humboldt also asserts, in a part of the paragraph which we have not quoted, that his great teacher exercised a surprising perspicuity in eliciting the truth from the confined narratives of travellers. Nothing so easy; and his pupils have also profited surprisingly by his example and his perspicacity. Nor any thing so easy as to support his system, or any system, by the same perspicacity. It was only to translate the language of any other geognost into his own, to know what the observer saw better than he knew himself, and the work was done. It is a fashion that is just as efficacious now. But we must take our leave of Werner, to return to M. Humboldt himself.

And we return to his 85th page, to his positive geognosy. This properly constitutes the book, and it is just such a collection of naked facts about rocks, as Kirwan wrote, or might have written, in his equally "admirable" work. If this comprises the whole of Positive Geognosy, we are very unfit reviewers of M. Humboldt's book, since we entertain somewhat different views about what is Positive Geognosy. In all the Sciences, there are things, objects, and there are relations, actions, causes, analogies; categories, in short of different kinds, which have generally been held essential parts of what is called a science. Perhaps, however, we have misapprehended the meaning of the term "geognosy," but we do presume, that it implies somewhat more than what we find here, and that the adjective term, positive, does not form our author's justification for what he has given us.

It is indifferent where we take an example of this geognosy. We should have taken the first, which comprises the history of granite, but it is too long for extraction, and it is beyond abridgment. If any of our geological readers choose to read it, we will permit them to judge for themselves, and shall not be very uneasy at the prospect of the judgment which they will pronounce between ourselves and the author, as to the general issue. They may select any other part, or read the whole; and if they can extricate any knowledge out of the mass of confusion, why then we shall envy them. Order, or system, there is none. We are dazzled with references to authors, and references to the new world, and hard names; and when we have laboured through ten pages of porphyry or thonschiefer, it is all to be done over again; and when it is done, we ask ourselves what it is all about.

In truth, we did expect some single passage as an example; but, as far as the 240th page at least, we have turned backwards and forwards in vain, and cannot extract one sole paragraph which comprises an entire within itself. The author seems always breathless with matter, and the matter is itself breath, wind. A single fact, well detailed and simply stated, and, if the author ever did reason, reasoned on, would have been worth whole pages of Conradswalde, Prausnitz, Mondragon, Goldlauter, Schwartz, Kiffhauser, Caxamarca, and the ten thousand other names which dazzle our eyes at every sentence, and transport us, in an instant, from Saxony to Chili, as if we had been sitting on Prince Houssain's carpet.

We really cannot see the purpose of all this geography and authority, unless it be to show that M. Humboldt has got a map of the world at his elbow, and is a great traveller. Cannot he see that this is nothing more than an affair of Habitats, and that when once it is ascertained that the general relations of rocks are analogous or similar throughout the globe, it is unnecessary to torment us with every spot of his two hemispheres. The young botanist fancies he has performed a vast feat in science, when he has published in some journal, a list of the plants in the king's park at Edinburgh, or in Norfolk. And it is a very fit occupation for a philosopher in the "Lovely Science." But that granite grows in the Fichtelgebirge, and at Tehuilotepetec, and that it consists, in both of mica, feldspar, and quartz, or of more mica and less feldspar, is not now very marvellous information; and it is still less necessary that we should be told of every "gisement" in M. Humboldt's hemispheres, and of all and every variety and variation of every thing that he has seen or not seen. Were any general laws deduced, we could understand a purpose in all this; but, as it is, it is egregious trifling, and trifling which pretends to knowledge. Foxes grow in Leicestershire and in Greenland, and so they do in India, and, for ought we care, they may grow upon Popocatepetl; but in what way does the Science of zoology profit by being told that their tails are an inch longer in Mexico than in Bengal, or that they are less odoriferous in Norfolk than at Santa Fé da Bagotá.

The article Porphyry, positively dances before our eyes; and, what with transition, and dolerite, and trachyte, and all else of this never ending subject, we are utterly unable to conjecture what he means to prove, or what he means. Ten sentences might possibly have told his meaning, they would have told ours; but he is not a man of ten sentences, and if he ever meant any thing, he has lost sight of it, suffocated under the rubbish of his talk.

But we have arrived at the 360th page, and still we have been unable to extract one quotation by which we might convey an idea of that talk. Our readers must really oblige us by undertaking a task to which we find ourselves unequal. It is a truth, and to us a sad one, that we really have read the book twice through, so that we cannot be accused of a breach of duty; and it is not less true and sad, that it was less intelligible the second time than the first. Passages, facts, particulars of all kinds, we might criticise beyond endurance, but for what end? If, in our general remarks, our readers should esteem us severe, we will say, read, understand, explain; and we may say too, as Johnson said to Garrick, "If I have told the truth, Davy, why dost thou snub me?"

But there are more last words, and they will require a few last words from us.

We have already alluded to an air of *pretension* which pervades our author's writings; and, among other matters, to his algebra, forming one, but by no means all of the modes in which it is displayed. We mean it for all whom it may suit, and not for him alone, when we say that this use of symbols and equations when the proportion can be given in common language, is either a piece of miserable affectation, or else, what is vulgarly called priggism, a quality for which we possess no genteeler term. He who makes use of x and y when he might say fifty and a hundred, or square and round, because he happens to have arrived as far as biquadratic equations, wishes to show the world that he knows what algebra is, and would fain be thought a mathematician. The mathematician who, similarly, abuses algebraic language is either a *prig*, or else has been so long versant in the conjuration of *differences*, that he has lost the power of thinking, and the use of his own language. In the last case alone, he is pardonable. The purpose of algebra, of symbols, is abbreviation; it is that we may acquire the power of condensing a long proposition into a small space, so that we may see all its relations at a glance. It is short hand and no more. If the proposition is not of a nature to require this, from its shortness, or other causes, algebraic symbols serve no purpose but to make a plain thing obscure, and to convey an air of mystery. He who thus uses them, instead of proving his knowledge of algebra, betrays his ignorance as well as his affectation; he shows, perhaps, that he knows what substitution is, or what is a simple equation; but he shows, too, that he is a mere mechanic in this art.

Now M. Humboldt, as might be expected, has invented a pasigraphy, "a pasigrafia geognostica," since he must also tell us that he has published in Spanish and in Mexico, or has "elevated himself to general ideas" on geognosy by means of α, β, γ, δ. "This method" also "is double, it is either figurative (graphic, imitative,) representing the superposed beds by parallelograms, placed one above the other; or *algorithmic* indicating the superposition of rocks, and the age of their formation, as the terms of a series." And here, also, he takes occasion to tell us that he has published a book "sur l'irritation de la fibre nerveuse," or "Versuche über die gereizte Muskel und Nervenfaser," by which means we know that he writes in German as well as Spanish and French, and understands the nervous system as well as he does economy, geognosy, and astronomy.

"I published," "I had," "I did," I, the Great, meets us every where; and we believe, indeed, that there is not one of his books which does not contain an enumeration of all the prior ones, under some pretence or other. We have not the least objection that he, or any man, should write on all subjects, provided he understands them; but when we see this palpable betraying, we draw the natural conclusion that he does not write from the overflowings of his mind, but because he would be thought capable of writing on every thing. As to the purpose of this geognostical pasigraphy, or any purpose to be effected by it, we really cannot explain them, and shall therefore extract one passage, that our readers may form their own judgments.

"If the letters of the alphabet represent these superposed rocks, of two series,

$$\begin{matrix} \alpha, \beta, \gamma, \delta \ldots. \\ \alpha, \alpha\beta, \beta, \beta\gamma, \gamma, \delta \ldots., \end{matrix}$$

the first indicates the succession of simple and independent formations; granite, gneiss, mica-slate, clay-slate or muschelkalk, sandstone of Königstein (quadersandstein), Jura limestone, and green sandstone with lignite (below the chalk). The second indicates the alternation of *simple* with *complex* formations; granite, gneiss-granite, gneiss, gneiss-mica-slate, clay-slate (pp. 86. 88.); or, to give an example taken from the transition formation (p. 129.), limestone with orthoceratites, limestone alternating with slate, transition slate alone, slate and grauwacke, grauwacke alone, transition porphyry ..... In the *complex* formations, that is, in those which present the periodical alternation of several beds, we sometimes distinguish three different rocks, which do not pass to each other in the same group;

$$\text{or} \begin{matrix} \alpha, \beta, \alpha\beta\gamma, \gamma \ldots. \\ \alpha\beta\gamma, \alpha\beta\delta, \beta\alpha\varepsilon \ldots.., \end{matrix}$$

as in primitive formations, there are alternating beds of granite, gneiss, and mica-slate : so in the transition formation, alternating beds of grauwacke, slate, and limestone, or grauwacke, slate, and porphyry, or clay-slate, grauwacke, and greenstone, constitute one formation. In the transition rocks, as we have stated above, clay-slate and grauwacke alone are not the terms of the series. Those terms are all complex; they are groups, and grauwacke belongs at once to several of those groups. It thence results that the term *grauwacke formation* relates only to the predominance of that rock in its association with other rocks.

"Each class affords the example of independent formations which *prelude* as subordinate beds. If $\alpha\beta\gamma$, or $\alpha\beta$, $\beta\gamma$ indicate the complex formations of granite, gneiss, and mica-slate, or of granite and gneiss, clay-slate and porphyry, porphyry and syenite, marl and gypsum, that is, the formation in which beds of two, and even three rocks alternate indefinitely; $\alpha + \beta$, $\beta + \gamma$, will indicate that gneiss forms simply a bed in granite, porphyry in slate, &c. Then

$$\alpha, \alpha + \beta, \beta, \beta + \gamma, \gamma \ldots.$$

expresses the curious phenomenon of formations which *prelude* or announce themselves as subordinate beds. Those beds sometimes call to mind the terms that precede (*lower rocks*) sometimes the terms that follow (*upper rocks*). Thus we shall have

$$\alpha, \beta, \beta + \alpha, \beta, \beta \times \gamma, \gamma \dots$$

The porphyries and granular syenites of the transition formation penetrate into the red sandstone, forming subordinate beds. If the position of the formation of the valley of Fassa is such as has been lately announced (p. 340.), a preceding term (syenite) passes as far as into the alpine limestone or zechstein; it is the case in the series,

$$\alpha, \beta + \alpha, \gamma + \alpha, \delta \dots$$

"When we would apply the pasigraphic notation also to the elements of composed rocks, that notation may indicate, how by the progressive augmentation of the elements of the mass, and chiefly by the insulated crystals, beds are formed by a sort of *interior developement;*

$$abc, abc^2, abc^3, \dots abc + c.$$

"We have preferred in this particular case (beds of feldspar in granite, beds of quartz in mica-slate or gneiss, beds of hornblende in syenite, beds of pyroxene in transition dolerite) the letters of the Roman alphabet to those of the Greek, in order not to confound the elements of a rock (feldspar, quartz, mica, hornblende, pyroxene) with the rocks that enter into the composition of complex formations.

"We have hitherto shown that in making abstraction altogether of the composition and physical properties of rocks, the *pasigraphic notation* can reduce the most complicated problems of composition to great simplicity. This notation shows how the same subordinate beds (rock-salt in zechstein and red marl, §§ 28, 29.; coal in red sandstone, zechstein, and muschelkalk) pass across several formations, superposed on each other:

$$\alpha + \mu, \beta + \mu, \gamma, \delta + \mu \dots$$

It also reminds us of the return of feldspathic and crystalline formations in the transition rocks and red sandstone (Norway, Scotland), a return which is analogous to that of granite after gneiss and primitive mica-slate:

$$\alpha, \beta, \alpha, \gamma, \delta \dots \varkappa, \lambda, \alpha, \beta, \dots$$

"The first terms of the series re-appear, even after a long interval, after grauwacke and limestone with orthoceratites, that is, after *fragmentary* and *shelly* rocks."

Now we appeal to our readers, if we have judged M. Humboldt harshly, or whether this is not the very grimace of affectation. If he could reason for one moment, or if he had ever considered the real purpose and use of an arithmetical or algebraical pasigraphy, he would see that he was obscuring what he pretends to illustrate, and increasing labour instead of shortening it. A series summed in this manner, conveys no ideas; and if it is to convey any, the labours of the reader will be, to re-translate all that M. Humboldt has exerted so much useless labour in darkening. We only wonder that he has not given us his geognosy in hieroglyphics, that he might have tried to persuade us he was the rival of Young and Champollion. One advantage, indeed, we will not deny; and we are only sorry, therefore, that his whole hemispheres were not written in Algebra. Each method is equally free of the suspicion of conveying ideas, and the Greek would have been shorter than the French.

Geognosy, indeed, can now be contained in a nutshell, and by a shorter process than the Iliad was crammed into one. And lest our readers should doubt the possibility of this, we shall give them the whole matter of the "two hemispheres" in two lines. But unless we gave them the whole passage, they might suspect us of playing with their credulity, and of jesting with M. Humboldt. Here it is;

> "In order to unite the principal phenomena of the position of rocks in the primitive, intermediary, secondary, and tertiary formations, I propose the following series:
>
> $\alpha,\ \alpha\beta,\ \beta + \varpi,\ \beta\gamma,\ \gamma + \tau,\ \alpha,\ \gamma,\ \delta,\ \alpha,\ \beta,\ \delta,\ o,\ \|\ \varkappa^{g},\ \tau',\ \delta\tau',\ \delta',$
> $\delta' + \pi,\ \gamma,\ \tau',\ \sigma\pi,\ \sigma + \alpha,\ \sigma\pi,\ o\ \|\ \pi\varkappa^{a} + \xi,\ \tau^{a} + \vartheta,\ \varkappa^{n},\ \tau^{m},\ \varkappa^{q},$
> $\tau^{o},\ \varkappa^{l},\ \tau^{c},\ \|\ \varkappa^{21},\ \tau^{p} \ldots.$
>
> "It would be useless to give the explanation of those characters; it will be seen by comparing them with the table of formations. I shall confine myself to fixing the attention of the reader on the accumulation of porphyries (π) on the limits of the transition and secondary formations, the position of euphotide formations (o), the great deposites of coal and lignite (ξ), and on the return (almost periodical) of feldspathic formations of transition granite, gneiss, and mica-slate (α, β, γ). As the notation I here present may be variously graduated by the manner in which the characters are accentuated, in uniting them as coefficients in complex formations, or in adding exponents, I doubt whether the names of the rocks arranged by series at the side of each other would address itself as forcibly to the eye as the algorithmic notation."

We must now take leave of the book and the author. We must be sorry that we have been obliged to write such hard truths, and yet we fear that were the cause to occur again, we should do it again. We have said that it was a justice we owed to others, to show that M. Homboldt was not the only authority, or the highest, in every subject of science. And it is not fitting or just, that the public, which cannot judge, and which necessarily follows the cry of the day, should measure any man by such an imaginary standard, and set up a false God to worship, to the abasement of all others. It is an age of monopolies, but it is hard that the principle of monopoly should be extended from fish to fame, from tea and porter to geognosy and botany, and to all else which ought to be the common property of the republic (which we hope it will ever continue,) of science and letters.

To himself, we might suggest, that, with more accuracy, less *pretension*, better writing, and far less writing he might have acquired a fair share of permanent reputation; but he has desired to be more than all, and will live to see his honors melt from him as snow before the summer. To retrieve is perhaps now impossible; but if he shall shew any desire to do so, we will be among the first to allot the praise, which, he ought now to know, must follow solidity and accuracy, and can never long follow any thing else.

Art. VIII.—*Truitê élêmentaire de Minéralogie*, par F. S. Beudant. Paris. 1824. pp. 856.——*An Elementary Treastise on Mineralogy*, by F. S. Beudant. (W.)

Many of the remarks in the preceding article, are applicable to this work; the author is deeply infected with the mania of fabricating new names, and of unnecessarily multiplying genera and species; but as it contains much that is valuable to the mineralogist and artist, we shall proceed to offer some account of it.

M. Beudant is professor of Mineralogy in the academy of Sciences at Paris, and is the author of an elementary treatise on the physical sciences, a voluminous work on Hungary and various important and interesting memoirs which have been published in the scientific journals of Europe. The present work is a part of his course on the physical sciences, and will, we are led to expect, be followed by a treatise on chemistry. It is divided into four distinct parts or books.

The first book comprehends the facts and observations upon which the science of mineralogy is founded, the external characters, the physical properties, and chemical nature of minerals. The second book is devoted to an exposition of the peculiar system of classification and nomenclature adopted in the work. M. Beudant proposes to divide minerals into three groups or classes, for which he offers the following names; Gazolytes, Leucolytes, and Chroicolytes.

Under the first of these are included those families, the types of which have the common property of existing in an aëriform or gaseous state, or of combining with oxygen, hydrogen, or fluorine, and forming permanent gases,—hence the name Gazolytes, soluble in gas. The second group has also two characters common to the bodies which serve as types to the families, the one being negative and not soluble in gas; the other being positive and forming *colourless* solutions with acids; hence the name Leucolytes, expressive of this character. The third group includes those bodies which give coloured solutions. The following is a tabular view of this arrangement.

Gazolytes.	Leucolytes.	Chroicolytes.
Silicides,		Tantalides,
Borides,	Antimonides,	Tungstides,
Anthracides,	Stannides,	Titanides,
Hydrogenides,	Zincides,	Molybdides,
Azotides,	Bismuthides,	Chromides,
Sulfurides,	Hydragyrides,	Uranides,
Chlorides,	Argyrides,	Manganides,
Phtorides,	Plumbides,	Siderides,
Selenides,	Aluminides,	Cobaltides,
Tellurides,	Magnesides,	Cuprides,
Phosphorides,		Aurides,
Arsenides,		Platinides,
		Palladides,
		Osmiides.

The Silicides are composed of the oxide of silicium, either alone, or in combination with other oxides, and after fusion with potash afford a compound soluble in acids; the solution yielding on evaporation silica. The Borides are composed of boric acid either alone, or in combination with different oxides. Soluble in alcohol to which they impart the property of burning with a greenish flame.

The family of Anthracides comprises the carbonaceous substances, bitumens, organic salts, as mellite, oxalate of iron, and urate of lime or *guano*. To them succeed the carbonates of lime, of iron, manganese, zinc, barytes, strontian, lead, silver, bismuth, and copper.

The Hydrogenides, as the name of the family implies, yield water by the action of an alloy of potassium, and thus we have oxide of hydrogen, or water, &c. The nitrates come under the family of Azotides. The sulphurets compose the family of sulfurides, and are subdivided into simple sulphurets, and compound sulphurets, as "sulfo-arseniure de cobalt" or grey cobalt, sulphuret of copper and iron, copper pyrites. To these succeed the sulphates.

We have the same principles of nomenclature followed throughout the work, and the specimens given are sufficient to show that the system is intended to be a chemical one. If the successive discoveries in crystallography, as M. Beudant justly remarks, have freed mineralogy from the empiricism to which it had so long been subjected, it has likewise been brought under more fixed and certain laws, by the researches which have been made into the chemical nature and composition of minerals, within the past six years. These investigations he conceives seem not only to authorize, but to demand an entire new classification of minerals, and with a view of effecting this, the present work appears to have been undertaken. Mineralogists will of course oppose many objections to the proposed classification, but without professing to advocate this new system, we have only felt desirous of giving such a general account of the work as will make it known to our mineralogists. The descriptions of individual minerals which M. Beudant has given, are extremely concise, being for the most part confined to the predominating colour, the crystalline form, and the action of the blow-pipe, and of chemical agents.

As a specimen of our author's manner of describing substances, we may take the following.

"Gazolytes;—family, silicides; genus, silicates; division 1st. aluminous silicates.

Solution freed from silica, forming with solution of caustic potash, a precipitate more or less abundant; sometimes none; the supernatant liquor giving with hydrochlorate of ammonia an abundant gelatinous precipitate (*alumina*) insoluble in carbonate of ammonia, or partially soluble when *glucina* is present.

A. Simple aluminous silicates, a very large solution gives, in general, but a slight precipitate with solution of caustic potash.

SPECIES 1st. *Pinite.*

An opaque substance, tender, compact, or laminated, fusible; crystals derived from a rectangular prism.

Specific gravity 2.98.

Composition $= \dddot{A}\,\dddot{S}i^2$, 1 atom of alumina 2 of silica; or by weight,

Silica	65
Alumina	35
	100

This substance is often mixed with a greater or less quantity of hydrate of alumina or of hydro-oxide of iron.

Varieties.

Crystallized, cylindrical, laminated, lamellæ disseminated in rocks."

On the subject of crystallization, M. Beudant has noticed several curious facts, and given a new arrangement of crystalline forms, which he conceives are reducible to seven groups or types, viz. 1. The Tetraëdron. 2. The acute or obtuse Rhomboëdron. 3. The straight prism with square bases. 4. The straight rectangular prism. 5. The straight prism with bases of oblique-angled parallelograms. 6. The oblique prism with rectangular bases. 7. The oblique prism with an oblique-angled parallelogram for the base.

In the 6th chapter, M. Beudant proceeds to develope the causes, to the operation of which the great variety of crystalline forms are to be imputed; and which determine any mineral substance to assume one form, or the modifications of that form to the exclusion of others. The solution of these questions is one of the most interesting points in philosophical mineralogy, but it is one of those on which little is known. Our author made numerous researches on natural productions, and many experiments on the salts obtained in the laboratory, and was thus enabled to ascertain three causes having great influence on the crystallization of bodies. These are, 1st. Mechanical mixture of foreign matters in the liquid in which a salt crystallizes, which seems greatly to contribute to the simplicity and regularity of the form; this is also observable in natural crystals, as in those of oxinite, feldspar, &c. when mixed (*remplis*) with particles of mica, and which are more simple than purer crystals in their immediate vicinity.

Secondly, the nature of the liquid in which crystallization takes place, appears to have much influence on the forms, in various ways, sometimes changing them entirely, at others modifying them by additional facets. Common salt, for example, always crystallizes from its solution in pure water in the cubical form, but in crystallizing from a solution of boracic acid, it always, in the experiments of M. Beudant, assumed the form of a cube truncated on the angles, and consequently passing to the octoëdron. Alum crystallizes from pure water in the form of an octoëdron, but from muriatic acid in the form of the icosaëdron. Very obvious changes are also produced by the addition of a few drops of an acid solution.

As a presumptive proof that anologous principles have operated in nature, it is observed that the same mineral generally affects the same form when accompanied by the same substances, and on the other hand when associated with other substances it presents different forms. The arragonite, for example, which is found in iron mines, is always in very acute pyramids; that on the contrary which is found in the gypseous clay of saliferous deposites, is always in the form of prismatic crystals, grouped in the form of hexagonal prisms.

Thirdly, the substances with which a salt may happen to combine at the moment of crystallizing likewise determine remarkable modifications of the crystalline forms, sometimes reducing the crystals to their most simple forms, and sometimes multiplying the number of their faces. These modifications vary even in the same salt according to the nature of the body with which it is combined. Thus, for example, the sulphate of iron mixed with sulphate of copper, always assumes the form of an oblique rhomboidal prism: sometimes the quantity of sulphate of copper is not sufficient to produce this form, at other times it is sufficient to produce a very near approach to it. M. Beudant has given many illustrations, for which we must refer our readers to the work itself. The discovery of these curious facts promises to form an epoch in the history of Mineralogical Science.

The latter half of this volume is occupied by geological details, so far as they relate to the situation and mode of occurrence of the various minerals described in the preceding part. The second chapter contains a particular description of the substances which enter into the composition of rocks, or which occur in such large masses themselves, as entitle them to be considered as component parts. These are quartz, feldspar, mica, diallage, amphibole, idocrase, garnet, carbonate and sulphate of lime. The third chapter contains an account of the non-metallic substances which occur in deposites of small extent, as rock-salt, coal, lignite, and sulphur. To this succeeds a general account of the metalliferous deposites; of those substances which occur in the form of imbedded masses, veins, &c. and of those which are found in the sand and gravel of the newer formations, as the gems, &c.

The fourth book will be found interesting to others than mere mineralogists, as it is devoted to the consideration of the products of the mineral kingdom which are used in the arts, in architecture, jewellery, &c. The curiosity and interest which are excited by the beauties of nature, the investigation of the laws to which its productions are obedient, are unquestionably strong inducements to the study of natural history; but of what importance are all our discoveries, asks M. Beudant, to the mass of mankind?

It is the application of our knowledge of these laws and these productions, to the arts, and to the melioration of human existence, that ought to render them the great object of attention. The mineral kingdom probably furnishes the greatest number of productions which can be applied to useful purposes; offers many resources to the industrious, gives employment to a large proportion of men in civilized life, and yields the most wealth. Architecture draws from it the greatest proportion of its materials, not only what are essential to its structure, but much that is merely ornamental; agriculture derives from it important aid in the improvement of soils; the mechanic arts it supplies with metals, and a vast number and variety of rude materials; nor does it contribute less to the chemical arts, while even luxury daily makes no inconsiderable demands upon it. The number of human beings who derive their sole support from the mineral kingdom, is immense; in France alone, which is yet not on a level with its neighbours, the number of men employed in the quarries, mines, and conveyance of mineral substances, is more than three millions. If to these we add all those to whom the mineral kingdom furnishes the raw material, and whose existence depends on this supply, how great will be the number? According to the official reports, these natural products annually furnish an amount of wealth of 933 millions of francs, in rough metallic materials, which the manufacturers and artists increase a hundred fold by their industry, 130 millions of combustibles, more than 500 millions in stones for building, in sand, in earthy matters for the glass makers and potters, more than 100 millions in different matters for luxury and a multitude of daily wants. Is not all this sufficient to attract our attention to the kingdom which supplies them?

M. Beudant proceeds to take a rapid view of the substances thus employed, commencing with those which are used in building, with a few extracts from which we shall close this notice.

Use of Minerals in the Art of Building.

Though we may use almost every kind of stone in building, it is by no means the case in architecture, properly so called;—it is necessary in architecture, that the materials should be fine enough to admit of sculpture—that they be easily wrought without breaking, with pointed tools and with the chisel,—that they may retain the mouldings, that they be of such tenacity, that the lower parts of the edifice shall not be crushed under the weight of the upper parts; and that at the same time they be not too heavy. It is absolutely necessary also that the variations of the atmosphere have no destructive influence over them, that water not only shall not decompose them at last, but even that it shall not penetrate them at all, or rather, that it be not retained within them long enough to be frozen; for by being converted into ice, which occupies a larger extent, it would occasion ruptures in the rock. Finally, it is necessary that these materials be found in extensive beds, that these beds be homogeneous, and free from fissures, that they may be regularly quarried, so that stones of all dimensions may be procured from them.

All these qualities, which architecture imperiously demands, exclude from this species of labor the greatest part of the substances, which form great masses on the surface of the globe, and there are but four kinds, with a considerable number of varieties, that can be used, viz. calcareous rocks, sandstones, volcanic productions, and granitic rocks.

Calcareous Rocks.

The calcareous rocks are most in use, not only because they are most abundant, but because they generally have the advantage of being wrought, and sawed with more ease than any other kind, and at the same time have a sufficient degree of tenacity to resist pressure, to retain angles, mouldings, &c. The varieties are not employed indifferently; some of them have too little cohesiveness, as for example, chalk, in most of its varieties; many granular calcareous rocks, simple or micaceous, of the primitive and intermediate soils, which do not resist pressure. Others, although their parts are perfectly aggregated, are too brittle, too dry, according to the expressive term of the workmen; such are the very compact, fine-grained calcareous stones, with a conchoidal or scaly fracture; these varieties moreover are frequently full of seams, which diminish their solidity, whether they continue open or whether they have been filled up, and closed with calcareous spar, which has itself but little strength.

The calcareous rocks that have the greatest tenacity and that consequently are best suited to architecture, are generally the compact varieties, whose fracture is uneven, smooth, or irregular, of a dull or earthy lustre; and those that are formed of the remains of shells, confusedly thrown together, and united by a cement which is half crystalline, half earthy. These different varieties are most abundant in secondary and tertiary soils, in deposites analogous to those of the Jura and Paris. These formations have furnished most of the monuments of the civilized world, and the stones taken from them are often transported to a great distance. The finest houses of Amsterdam are built with stones from Schaumberg in Hesse. It is said that the mosques in Constantinople are paved with flag stones, taken from the quarries of Papenheim in Bavaria, on the banks of the Danube, &c. They exist likewise, but in less quantities, in intermediate and primitive soils, which also furnish some crystalline varieties with very small plates crossing one another, of very great tenacity, but which are almost always employed for ornamental purposes.

The rocks of the secondary formations are those usually employed in the south part of Lorrain, in Franche Compté, in Burgundy, in Bourbonnois, on the banks of the Rhone, in one part of Normandy, Poitou, &c. They are also common throughout Germany, and are to be found in some parts of England. The greater number consist of calcareous shells, of a yellowish white, or bluish gray colour, full of encrinites, which belong to the upper part of the Jura; but many are found in the lower parts, of a more compact texture, which at the same time is neither laminated nor too dry; from these may be obtained stones of large dimensions, which present many advantages. The latter were employed in preference to all others by the Romans, in those fine monuments, which still adorn many of the cities in the south of France. A similar stone was used in the construction of the famous Egyptian Pyramids.

* * * *

It is to be observed that most calcareous stones should be so employed in building, as to occupy the same position they had in the quarry from which they were taken; the reason of this is, that the greatest part of the calcareous stones found in beds more or less deep, are made up of small layers which are scarcely perceptible, but which would cause the stone to scale, or to split in a vertical direction, when placed in a different position from its former one in the quarry, or as it is termed, when it is placed in fault (*en delit*). No stones but such as are of a very compact structure, perfectly homogeneous, forming beds of a great depth, can be placed on either side indifferently.

* * * *

The Granitic Rocks.

The granitic rocks would not be employed in any manner, except in the want of every other kind of stone, or when it is intended to erect monuments as durable as the world; they have solidity that is proof against every thing; witness the numerous Egyptian monuments that have endured for ages, and are quite as fresh as though they were but just finished; but they are very hard, and consequently are hammered with difficulty; they form, at the surface of the earth, very large masses, without stratification, which circumstance indeed furnishes means of obtaining pieces as large in their dimensions, as may be desired; but for the same reason the process of working the quarries is laborious and expensive. Yet in many districts of France no other stone is employed in building; it is this stone that builders are obliged to make use of in a great part of Bretagne, and in Normandy, between Cherbourg and Alençon, in La Marcheana and the adjacent parts of Bourbonnois, Limousin, a part of Auvergne, of Lyonnois (Montbrisson &c.) of Burgundy (Autun &c.), in a great many countries of Europe, and other parts of the world. The quays of the Neva, and the canal of Catherine are of granite; the city and the fortifications of Rio Janeiro are built of granite, as are likewise the towers of the celebrated Chinese wall, &c. This rock is often transported at great expense to places where it is not to be found, either to be employed in union with other materials in the construction of colonnades for large edifices, in which case it is a material for ornamental purposes, or to be used, on account of its great solidity, in masonry that is intended to last forever; for the pavement of streets, quays, all places where the passing is constant, &c.

* * * *

There is but little choice among the granite rocks; all that is necessary is to avoid such as have become altered, those in which the constituent parts exist in large pieces, those in which the mica has a tendency to dispose itself in leaves, and those which are passing into gneiss.

In architecture, granites of different ages, and many kinds of rocks to which geologists give other names, are employed; such as the true porphyries, as in the vicinity of Roanne,* and sienites, as in *les Vosges*, and as seen in many of the Egyptian monuments. Sometimes the name of granite has been given even to the arenaceous rocks of deposites of sea sand-stone, in which all the elements of granite may be found re-aggregated together.

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* Our porphyry at Malden is of this kind. W.

* * * *

Mortar.

Sometimes the different stones, that enter into the construction of a building, are squared with much care on the faces by which they are to join, and are then applied the one upon the other without any intervening substance; but they are generally joined by means of a soft substance, capable of becoming hard after their junction, which is known by the name of cement or mortar. There are many kinds of cement; in the coarse structures of the country, nothing more than clay moistened with water is usually employed, but in more elaborate structures the cement is more carefully selected. It appears that in Egypt, bitumen was often used for that purpose, and we are assured that the walls of Babylon were made of bricks, cemented together by that substance. It has moreover been introduced of late into use in France for small structures, for the common sewers of great cities, for basins, &c. From time immemorial lime has been used, mixed with different substances, reduced to powder, in order to compose the mortar which is to unite the stones in great monuments, and in structures that are intended to possess great solidity. In many places, plaster is used as in the lining of walls, in rough-cast, and in a multitude of cases.

Lime is the result of the burning of the carbonate of lime, an operation which takes the name of calcination, and is extended to many other cases, in which is involved the reduction of a metal, of a sulphuret, or a salt to an oxide. It may be obtained from all the varieties of limestone, but they do not all yield it equally suitable to all kinds of use. In general, lime may be divided in the arts into three kinds, fat, dry, and hydraulic lime. The first of these is inferior to the other two; it absorbs a great quantity of water in slacking, it is long before it hardens even in the open air, and under water, or in moist situations it does not harden at all; yet this kind is that most extensively used, partly perhaps, because it is the most common, and partly because, as it admits of a large quantity of sand, and absorbs a great deal of water, it furnishes much more mortar to a given weight or measure of lime. It is accordingly the most economical, but its cheapness is obtained at the expense of solidity.

We say dry lime is much better suited for building, because it hardens readily in the air, and even in moist places, but it is less economical, for it absorbs but little water, and admits only a small proportion of sand.

Finally, hydraulic lime hardens, not only in moist places, but under water, without the aid of any other substance; it is consequently the last thing that can be employed in all hydraulic structures, in foundations that are situated in wet places, &c. * * * *

It has been thought for a long time that in the drying of a cement, the lime attracts carbonic acid from the atmosphere, and that the mass acquires its greatest solidity when the whole is converted into a carbonate. It is for this reason, it has been said, that the cements which we find in the ancient Roman monuments, possess at this day a greater solidity than our own; but this is an error; the Roman cements do not contain the quantity of carbonic acid necessary to saturate the lime; and hence it is evident, that there exist silicates and aluminates of lime, which are formed at a time posterior to the use of the substance. It is even to be remarked that the hardness of the ancient cements is greater in proportion as the quantity of carbonic acid is less. These observations may serve to show how important it is in choosing, to give the preference to siliceous and aluminous limestones, or when these cannot be procured, to prepare the compounds, that it is necessary to introduce, with great accuracy.

Art. IX.—*A New Theory of Parallel Lines.* By Warren Colburn, Esq.

The following proposition, on which the theory of parallel lines in its present form depends, has always been considered as not sufficiently evident to be taken as an axiom; but no demonstration has ever been given sufficiently simple to be introduced into an elementary treatise. Consequently, all elementary treatises are defective in this point.

Proposition. If a straight line meets two straight lines so as to make the two interior angles on the same side less than two right angles, these straight lines being continually produced, will at length meet upon that side on which the two angles are less than two right angles.

Upon this, Simson remarks, "the proposition which is usually the 5th postulate or the 11th axiom, by some the 12th, on which this 29th depends, has given a great deal ado to geometers. It seems not properly placed among the axioms, as indeed it is not self-evident, nor does it admit of a demonstration in a strict sense, &c." Simpson's Euclid, Note to Prop. 29th, Book I.

Lacroix, in his geometry, in a note to No. 40, thus remarks. The imperfection of the theory of parallel lines, resides in the difficulty of proving directly this proposition. Many authors have made unsuccessful attempts to prove it; and others, as Bezout, have endeavoured to conceal the faults of the reasoning; which seems to me contrary to the rigorous duty which every author of elementary works imposes upon himself, &c.

Lacroix gives in a note, a demonstration by M. Bertrand, but it is not sufficiently elementary for learners.

Legendre gives a kind of mechanical illustration of it in the text, and an analytic demonstration sufficiently difficult in a note.

In examining the subject, it appeared to me that the difficulty arose from the definition which did not seem to be the most general or the most natural. I therefore changed the definition for the one given below; which is more general, for the old one follows as a corollary from it, and I believe it will generally be granted to be more natural.

I then endeavoured to prove the usual propositions relating to parallel lines, without using the contested proposition, leaving that for a later period when I should have more data. How well I have succeeded, mathematicians must decide.

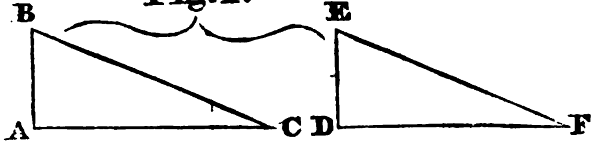
My demonstrations are not longer or more difficult than those usually found in elementary works on the same subject. After my own demonstrations relating to parallels, I have introduced several propositions from Lacroix, in order to obtain data to prove the contested proposition, which is inserted last. The whole so corresponds with Lacroix, that it might be inserted into his work, by omitting such of his demonstrations as relate to the same points, and placing No. 50, 61, and 62 after the proof of the disputed proposition.

A series of propositions might be selected from Legendre, by which the disputed proposition might be proved, using my demonstrations first, but it would destroy the present arrangement of the work.

1. If in any two triangles *ABC* and *DEF*, (fig. 1.) right-angled in *A* and *D*, the sides *BC* and *EF*, opposite the right angles be equal, and a side *AB* in the one, equal to a side *DE* in the other, the triangles are equal.

Demonstration. If *AB* be laid upon *DE* so that *A* may coincide with *D*, the point *B* will coincide with *E*, and the line *AC* will coincide with *DF* on account of the equal angles *A* and *D*. Now *BC*, being equal to *EF*, must meet *DF* at the same distance *DF* from the perpendicular *ED*. Therefore, *AC* is equal to *DF*, and the angles *B* and *C* to *E* and *F* respectively.

Fig. 1.



2. If in any two triangles *ABC* and *DEF* (same figure,) the sides *BC* and *EF*, opposite to the right angles, be equal, and one angle as *C* in the one be equal to the angle *F* of the other, the two triangles are equal.

Demonstration. If *BC* be laid upon *EF* so that *C* may coincide with *F*, the point *B* will coincide with *E*; and the angle *C* being equal to *F*, the line *AC* will coincide with *DF*. Now from the point *E*, *only one perpendicular to the line* DF *can be drawn.* Therefore the line *BA* will coincide with *ED*, and be equal to it. Also *AC* will be equal *DF*, and the angle *B* to the angle *E*.

Definition. *Two straight lines so situated as to be equidistant throughout their whole extent, are called parallel lines.*

Corollary. Parallel lines, if produced to any extent in either direction, will not meet.

It is proposed to prove the following propositions relative to parallel lines.

When two parallel lines *AB* and *CD* (fig. 2.) are cut by a straight line *EF*:

1st. *The alternate internal angles are equal.*[*]

2d. *The alternate external angles are equal;*

3d. *The corresponding angles are equal;*

4th. *The internal angles on the same side are together equal to two right angles.*

5th. *The external angles on the same side are together equal to two right angles.*

6th. *When two straight lines are so situated as to have either of the above properties, they are parallel.*

Demonstration. 1st. When the line *EF* is oblique. From the points *H* and *I* where the line *EF* cuts *AB* and *CD*, draw *HK* perpendicular to *CD*, and *IL* perpendicular to *AB*.

In the triangles *HIK* and *HIL*, right-angled in *K* and *L*, the line *HI* is common, and opposite to the right angle in each, and the line *HK* is equal to *IL* by the definition; therefore the two triangles are equal in all their parts. Hence

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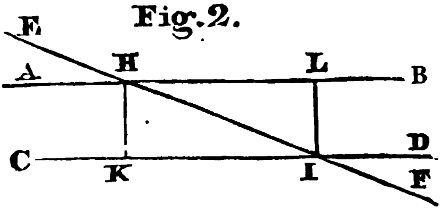
[*] The names of the angles, adopted here are those of Lacroix. They will be easily understood by referring to the figure.

Fig. 2.



1st. The angle $HIK = IHL$; (these are *alternate internal angles.*)

The angle $HIK + HID =$ two right angles, also
The angle $IHL + IHA =$ two right angles; hence
The angle $HIK + HID = IHL + IHA$;
Taking away the equal angles HIK and IHL, there remains $HID = IHA$; (these are also alternate ininternal angles.)
Therefore the alternate internal angles are equal.

2d. and 3d. The angle $AHE = IHL = HIK = DIF$, and
The angle $EHL = AHI = HID = FIK$; therefore,
The alternate external angles are equal, and the corresponding angles are equal. (The angles EHL and HID and all others situated like them, are *corresponding angles.*)

4th. The angle $AHI + IHL =$ two right angles; but $HIK = IHL$, therefore, $AHI + HIK =$ two right angles; that is, the internal angles on the same side are together equal to two right angles.

5th. The angle $AHE + KIF = HIK + AHI =$ two right angles; that is, the external angles on the same side are together equal to two right angles.

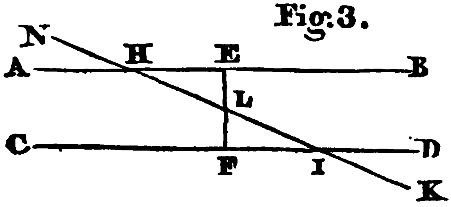
6th. In the same figure, let the straight lines AB and CD be so situated, that, when cut by the straight line EF in any two points H and I, the two interior angles on the same side AHI and HIC may be together equal to two right angles.

From the points H and I, draw HK perpendicular to CD, and IL perpendicular to AB.

The angle $AHI + HIC =$ two right angles by hypothesis, also the angle $AHI + IHL =$ two right angles; therefore, the angle $IHL = HIC$. Hence the triangles HIK and HIL right-angled in K and L, have the angles HIK and IHL equal, and the line HI common and opposite to the right angles; therefore the two triangles are equal in all their parts. Hence the line $HK = LI$. The lines AB and CD, being equidistant in the two parts HK and LI, must be equidistant throughout; that is, they are parallel.

2d. When the line *EF* is perpendicular to one of the lines;

Fig. 3.



Let *AB* and *CD* (fig. 3.) be two parallel straight lines, and cut obliquely by the straight line *NK* in the points *H* and *I*. From *L* the middle of the line *HI* draw *LE* perpendicular to *AB*, and *LF* perpendicular to *CD*. In the triangles *LHE* and *LIF*, right-angled in *E* and *F*, the angle $LHE = LIF$, being alternate internal angles, and the lines *LH* and *LI*, opposite to the right angles, are equal by construction, therefore the two triangles are equal, and the angle $HLE = ILF$.

The angle $HLE + ELI =$ two right angles, and because the angle $HLE = ILF$, the angle $ILF + ELI =$ two right angles. Therefore the line *EF* is a straight line and perpendicular to both *AB* and *CD*, from which it will follow that the properties, which were proved for the oblique line, are true for the perpendicular.

The Problem for drawing parallel lines may follow here.

Theorem.

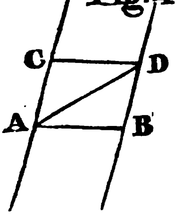
The three angles of a triangle are together equal to two right angles.

For a demonstration, see any treatise on geometry.

Theorem. Lacroix, No. 54.

The parts *AC* and *BD*, (fig. 4.) of two parallel straight lines intercepted between two parallel straight lines are equal to each other, and reciprocally.

Fig. 4



Demonstration. If the line *AD* be drawn, two triangles *ABD* and *ACD* will be formed, which will be equal; for taking *AD* as secant to the parallels *AB* and *CD*, the angles *BAD* and *ADC* are equal, being alternate internal angles; taking *AD* as secant to the parallels *AC* and *BD*, the angles *ADB* and *DAC* are equal for the same reason; farther, the side *AD* being common to the two triangles *ABD* and *ACD*, the triangles are equal. Therefore the sides *AC* and *BD* are equal, which was to be demonstrated. The same is true of the sides *AB* and *CD*.

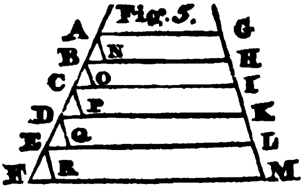
Reciprocally, if the parts *CD* and *AB* are equal, and also the parts *AC* and *BD*, the triangles *ABD* and *ACD* will have their three sides equal each to each, and will consequently be equal. The equality of the alternate internal angles *CAD*, *ADB* establishes the parallelism of the straight lines *AC*, *BD*, and the equality of the angles *BAD*, *CDA*, that of the straight lines *AB*, *CD*.

Theorem. *Lacroix*, No. 56.

If any two straight lines *AF* and *GM* (fig. 5,) are so situated as to be cut by a number of parallels *AG*, *BH*, *CI*, &c. in such a manner that the parts of *AF* intercepted between them are equal, the parts *GH*, *HI*, *IK*, &c. of the second will also be equal to each other. The demonstration is varied a little from that of Lacroix.

Demonstration. Through the points *A*, *B*, *C*, &c. draw the right lines *AN*, *BO*, *CP*, &c. parallel to *GM*, the triangles *ABN*, *BCO*, *CDP*, &c. will be formed, in which the sides *AB*, *BC*, *CD*, &c. are equal by hypothesis; the angles *ABN*, *BCO*, *CDP*, &c. are equal, being corresponding angles with regard to the parallels *BH*, *CI*, *DK*, &c., cut by *AF*; the angles *BAN*, *CBO*, *DCP*, &c. are equal, being corresponding angles with regard to the parallels *AN*, *BO*, *CP*, &c. cut by the line *AF*. Consequently the triangles *ABN*, *BCO*, *CDP*, &c. are equal; from which it follows that the lines *AN*, *BO*, *CP*, &c. are equal to each other. But the lines *GH*, *HI*, *IK*, &c. are respectively equal to *AN*, *BO*, *CP*, &c. being parallels intercepted between parallels, therefore *GH*, *HI*, *IK*, &c. are equal to each other.

Fig. 5.



Corollary. *Lacroix*, No. 57.

It follows from what precedes, that *AB* is contained in *AF*, as many times as *GH* is contained in *GM*; hence we have this proportion, (or equation.)

$$\frac{AF}{AB} = \frac{GM}{GH},$$

or multiplying by *GH*, and dividing by *AF*,

$$\frac{GH}{AB} = \frac{GM}{AF};$$

Also,

$$\frac{2GH}{2AB} = \frac{GM}{AF};$$

$$\frac{3GH}{3\,AB}=\frac{GM}{AF}.$$

That is, any number of parts of *AF* bears the same proportion to the same number of parts of *GM*, that the whole line *AF* bears to the whole line *GM*.

Theorem. *Lacroix*, No. 58.

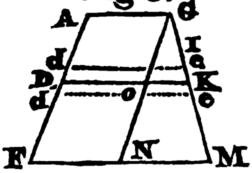
If three parallels *AG*, *DK*, *FM*, (fig. 6,) be cut by any two straight lines *AF*, *GM*, the parts intercepted between the parallels will be proportional, so that

$$\frac{AD}{DF}=\frac{GK}{KM}.$$

Demonstration. This proposition admits of two cases. 1st. Let *AD* be commensurable with *AF*, that is, let them be such that the ratio of *AD* to *AF* may be expressed exactly by numbers. Suppose, for example, that

$$\frac{AF}{AD}=\frac{47}{25};$$

Fig. 6.



If we conceive the right line *AF* divided into 47 equal parts, *AD* will contain 25 of those parts, and *DF*, 22. Drawing then through all the divisions right lines parallel to *AG*, the right line *GM* will be divided into 47 equal parts, of which 25 will compose *GK*, and 22 will compose *KM*; hence

$$\frac{AD}{DF}=\frac{25}{22},$$

$$\frac{GK}{KM}=\frac{25}{22},$$

from which it follows, that

$$\frac{AD}{DF}=\frac{GK}{KM}.$$

Farther, because of the equations

$$\frac{AF}{AD}=\frac{47}{25},$$

$$\frac{GM}{GK}=\frac{47}{25},$$

we obtain

$$\frac{AF}{AD}=\frac{GM}{GK}.$$

2d. If AF and AD are incommensurable, it may be proved as follows, that their ratio can be neither smaller nor larger than that of GK to GM.

Suppose, first, that

$$\frac{AF}{AD} = \frac{GM}{GI}, \text{ which gives } \frac{AF}{GM} = \frac{AD}{GI},$$

GI being smaller than GK. The side AF may always be divided into parts so small, that lines being drawn through all the points of division parallel to FM, one of them de will pass between the points I and K; we shall have according to what precedes, on account of the commensurability of AF and Ad,

$$\frac{AF}{Ad} = \frac{GM}{Ge},$$

or

$$\frac{AF}{GM} = \frac{Ad}{Ge}.$$

But by hypothesis

$$\frac{AF}{GM} = \frac{AD}{GI},$$

therefore

$$\frac{AD}{GI} = \frac{Ad}{Ge},$$

or

$$\frac{AD}{Ad} = \frac{GI}{Ge},$$

which result is absurd, for AD is greater than Ad, while GI is less than Ge.

By a similar process, it may be shown that we cannot have

$$\frac{AF}{AD} = \frac{GM}{GI'},$$

GI' being greater than GK. Therefore, since GI can be neither greater nor less than GK, it must be equal to it.

From the equation

$$\frac{AF}{AD} = \frac{GM}{GK},$$

we derive

$$\frac{AF - AD}{AD} = \frac{GM - GK}{GK},$$

or

$$\frac{DF}{AD}=\frac{KM}{KG},$$

or inverting terms

$$\frac{AD}{DF}=\frac{GK}{KM}.$$

Note. If the denominators of the two members are subtracted from the numerators, the numerators will each contain the denominators one time less than before.

1st. *Corollary.* *Lacroix*, No. 59.

If through the point G, the line GN be drawn parallel to AF, we have

$$GO=AD,\quad ON=DF,$$

being parallels intercepted between parallels; and by what precedes

$$\frac{GO}{ON}=\frac{GK}{KM},$$

$$\frac{GO}{GN}=\frac{GK}{GM}.$$

If, therefore, there be drawn in a triangle a right line OK, parallel to one of its sides NM, the two other sides GN and GM will be cut into proportional parts by the right line.

2d. *Corollary.* *Lacroix*, No. 60.

Reciprocally, when a right line cuts two sides of a triangle into proportional parts, it is parallel to the third side.

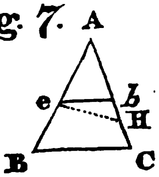
If in the triangle ABC, (fig. 7.) we have

$$\frac{AB}{Ae}=\frac{AC}{Ab},$$

and if eb is not parallel to BC, we can draw through the point e a right line eH parallel to BC, which would give

$$\frac{AB}{Ae}=\frac{AC}{AH}.$$

Fig. 7.



From the two equations, it appears that $AH=Ab$, consequently the lines eb and eH coincide with each other, and that the first is necessarily parallel to BC.

Lacroix, No. 63.

Definition of similar triangles.

Theorem. *Lacroix*, No. 64.

When two triangles *ABC* and *DEF* (fig. 8.) have their angles equal each to each, their homologous sides are proportional, and they are consequently similar.

Demonstration. If we take upon *AB* and *AC*, two parts *A e* and *A b*, respectively equal to *DE* and *DF*, and draw *e b*, the triangles *eAB* and *EDF* will be equal, since by hypothesis the angle *A* of the one is equal to the angle *D* of the other, and the sides *A e* and *A b* are equal to *DE* and *DF* by construction. The angle *A e b* being equal to *E*, will consequently be equal to *B*, and *e b* will be parallel to *BC*, we shall therefere have this equation,

$$\frac{AB}{Ae} = \frac{AC}{Ab} \text{ or } \frac{AB}{DE} = \frac{AC}{DF}.$$

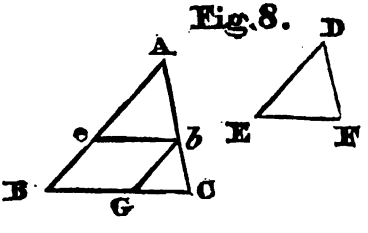
Drawing, then, *GB* parallel to *AB*, we have

$$\frac{AC}{Ab} = \frac{BC}{BG} \text{ or } \frac{AC}{DF} = \frac{BC}{EF},$$

since *BG* is equal to *e b*, which is equal *EF*. Since the member $\frac{AC}{DF}$ is common to both equations, we have

$$\frac{AB}{DE} = \frac{AC}{DF} = \frac{BC}{EF}.$$

Fig. 8.



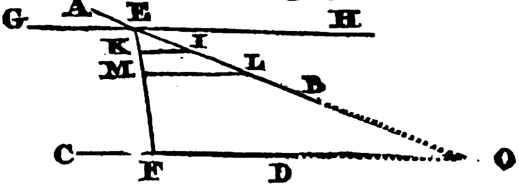
From which it results, that the homologous sides of the two triangles are proportional.

Corollary. Lacroix, No. 65. It follows from the preceding proposition, that two triangles are similar when they have two angles equal each to each, since the third angle in the one is necessarily equal to the third angle of the other.

Theorem.

If two straight lines *AB* and *CD*, (fig. 9.) cut by the straight line *EF* make the angles *BEF* and *DFE* together less than two right angles, they may be produced until they meet.

Fig. 9.



Demonstration. Through E draw GH parallel to CD, also from any point I in the line AB draw IK parallel to CD. The line IK will meet EF in the point K between E and F, for the angles HEF and DFE are together equal to two right angles, being the interior angles on the same side with regard to the parallels GH, CD; and the angles BEF, DFE, are together less than two right angles by hypothesis, hence the angle BEF is less than HEF and the line EB falls within the parallels GH, CD, consequently IK, being parallel to GH, CD, is between them, and a triangle EIK is formed. Through another point L of the line AB, draw the line LM parallel to CD which will meet EF in M, and form the triangle ELM, having its angles respectively equal to those of EIK, consequently these triangles are similar, and we have this equation;

$$\frac{EI}{EL} = \frac{EK}{EM}.$$

If EL is twice EI, EM will be twice EK and so on; that is, the line EB, as it is produced, approaches the line CD produced, exactly in proportion to its length; consequently they may be produced until they meet.

If the ratio of EK to EF be expressed by two numbers, a and b, so that

$$\frac{a}{b} = \frac{EK}{EF},$$

we have

$$\frac{a}{b} = \frac{EI}{EO},$$

which gives

$$EO = \frac{b}{a} \times EI.$$

Whence the point O of their meeting may be found.

General Intelligence.

Pastorff on the Solar Spots and Clouds.—In examining the sun's disk with a fine six-feet achromatic telescope of Fraunhofer, with powers varying from 25 to 400, M. Pastorff of Buchholtz, near Frankfort on the Oder, has observed several interesting phenomena relative to the spots on its surface, their penumbræ, and the phosphoric clouds. He observes that the penumbræ of the spots resemble a mass of the empty eggs of the *Bombix neustria*, which surround the black spots concentrically and with different breadths. These apparent eggs are contiguous, and, as it were, agglomerated the one to the other, with openings extremely small. M. Pastorff considers it quite certain that these spots with the penumbræ are on the surface of the solar globe, and that they disappear when the phosphoric clouds cover them, principally when they are near the margin of the sun; and he thinks that it is probably these phosphoric clouds, which, in the interval of some hours only, form this great variety of spots. On the 1st December, 1823, M. Brioschi of Naples observed a large spot equal to 1½ our globe, surrounded with an irregular and branching elevation, into which there seemed to be precipitating great masses of fire. The whole surface of the sun he saw like an ocean on fire agitated by a storm. M. Pastorff saw this same spot on the same day, when the phosphoric clouds were in great motion; but though he has often seen the agitation of the phosphoric clouds much greater, he did not consider it as resembling an ocean on fire. Almost always, when the spots approach the margin of the sun's disk, they divide themselves into several groups, or they re-unite if they have been previously subdivided. Very near the margin, the spots appear totally altered, and they almost always appear as if they were dissolved and changed into luminous clouds, though that dissolution is only apparent; for it is quite evident that, in proportion as these spots approach the margin of the disk, the penumbra or the nebulosity which encircles them, covers them more and more till they totally disappear. There is then only seen the luminous nebulosity, which is sometimes surrounded with phosphoric clouds. The sun always appeared more bright at its centre than towards its edge.—*Edin. Jour. of Science.*

Luminous Snow-storm.—Towards the latter end of March, in 1813, a shower of snow fell on Lochawe, in Argyleshire, which alarmed or astonished those by whom it was witnessed, according as they were influenced by superstition or curiosity. The day had been calmly beautiful, but the sky became suddenly gloomy, and in a few minutes a shower of snow fell. The lake (which was of glassy smoothness) immediately presented a luminous surface, forming one huge sheet of fire. A boat, in which were several gentlemen, presented a similar appearance, and the exposed parts of the bodies of the gentlemen seemed to the eye to burn, although without any feeling of warmth. When they applied their hands to any of the melting snow, the luminous substance adhered to them, as well as the moisture, and this property was not lost by the snow for twelve or fifteen minutes.—*Edin. Philos. Jour.*

In the 4th volume of the Memoirs of the American Academy is an account by Prof. Farrar of similar electrical phenomena observed during a snow-storm on the evening of the 17th of January, 1817. This storm extended through a great part of the United States, and was accompanied in some places with almost incessant thunder and lightning. Three persons, crossing the bridge over the Androscoggin river, observed the borders of their hats to be luminous, and when they held up their hands covered with woollen gloves, the ends of their fingers were also luminous. At Williamstown, Vermont, the luminous appearance was seen by a physician on the ears and hair of his horse's head, on the whip of a young man who accompanied him, and on the hat of a gentleman, who, in attempting to brush it off, saw it extend itself over the greater part of his hat. These electrical phenomena were also observed upon the fences and bushes, and upon all substances which extended to any considerable distance above the surface of the ground, and approached to a point, but they had not all the same form. For the most part the light appeared like a spark or star, but in some instances it resembled a blaze, in form of an inverted cone of about two inches in height, and three fourths of an inch in diameter at the base. These blazes emitted a hissing sound, resembling that of the water in a teakettle just before it boils, which could be heard distinctly at the distance of 6 or 8 feet. They were observed on stakes in the fences, higher than any other substances near them, and when they were removed, the blazes disappeared, and were succeeded by stars.

Appearances similar to the above were said to have been observed in other places on the 10th of March, 1817, and in one instance the light appeared on a well-pole when elevated; upon drawing down the pole, the light gradually diminished, and at length entirely vanished. When the pole was raised again, it reappeared.

In the 1st volume of the Memoirs is an account of a still more luminous effect noticed by Mr L. Baldwin at Woburn, Mass. during a thunder shower in July, 1771. Mr Baldwin having raised an electrical kite, became surrounded with "a fiery atmosphere," which produced a sensation of weakness in his limbs and "a kind of listless feeling." On returning to his house he found his family somewhat alarmed, and was informed that he appeared (during the flying of the kite) to be in the midst of a large, bright flame of fire, attended with flashings; and that they expected, every moment, to see him fall a sacrifice to the flame.

Zircon, a Phosphate of Yttria.—Extract from a letter from M. Berzelius.—I have been tempted, in the course of experiments, which I had undertaken in order to determine the composition of zircon, to ascertain whether thorina was not, as I had for a long time suspected, a hitherto unknown combination of zircon. With this object I employed the last portion remaining in my possession of a specimen, and I have discovered that this pretended earth is nothing more than a *phosphate of yttria* with an excess of the base. By a simple trial with the blow-pipe, I was able to discover the phosphoric acid.

M. Adolph Karl de Weisenfels has discovered that the infusion of one part of white mustard in eight parts of water, is an excellent reagent to discover the presence of alcalis, to the solutions of which it imparts a yellow color.—*Trommsdorff's N. Journal.*

WAERME, LICHT, COMETENSHWEIF, ABERRATION UND MESSURE DER ENTFERNUNGEN DER STERNE.——*Heat, Light, and Train of Comets, Aberration, and Measure of Distances of Stars.* By J. Schutz. *Stegermark, Zeitschrift*, p. 90.—The author examines the hypotheses which explain the light and heat at the surface of the earth, the cold and darkness in the higher regions of the air. The reflection of solar rays at the surface of the earth, in his opinion, contributes but little to the increase of light and heat in low situations; he appears to think that the increase is still less owing to the internal heat of the earth. Here is another hypothesis: every transparent substance, cut into the shape of a lens, refracts the solar rays, so that they are concentrated behind the substance. The atmosphere forms a fluid sphere around the earth, and performs the office of a lens for each point of the earth. The solar rays move in right lines; the farther they may be from the focus, the more they diverge from each other; they become rarefied, feeble, and finally lose their effect. If they fall upon the convex atmosphere of our globe, they are refracted, and concentrated, conformably to the laws of dioptrics, and the farther they penetrate into the atmosphere, the more are they refracted. Thus we may account for the fact, that there is a greater degree of light and heat in low situations, whilst it is always cold and dark in elevated regions. This theory, adds M. Schutz, explains the origin of the trains of comets. These bodies have no train when they are very distant from the sun. The nearer they approach, the larger is the train; moreover this last is always on the side opposite to the sun. Comets are round, aqueous bodies, which receive the light of the sun. The solar rays which fall upon the comet must, by the laws of refraction, become concentrated upon the opposite part of this transparent sphere. This occasions a degree of heat which reduces the water to steam. Above the ether, which may extend itself to a great distance upon our atmosphere, there must be a space devoid of air. Nevertheless, if the water of the comet is converted into vapour in this space devoid of air, by the solar rays, concentrated on one side, this vapour must necessarily move with great rapidity, and appear to us in the form of a train receiving the sun's light. As the concentration of solar rays is always opposite to the sun, the tail of the comet, which always appears on the side opposite to that body, disappears when the comet is so far from the sun that the solar rays are rarefied to such a degree, that they lose their agency.

Application of this theory to aberration. M. Schutz mentions the old explanation and that of Lichtenberg, which he endeavours to refute; then he adds, according to the preceding theory of light, the rays are made to converge by our atmosphere; a star must be seen in the direction these rays have, when they fall upon the eyes. An object seen through a lens may appear out of its actual place; this is the case with the stars, from the effect of the lens of air through which we see them. In the last place, says the author in concluding, this convexity of the atmosphere renders an exact estimation of the distances and magnitudes of the heavenly bodies impossible; for after the rays have been refracted, it is impossible to have the *real* distance or magnitude of any object.

It is unnecessary that we should add, that we merely report the opinions of M. Schutz, without adhering to them at all.—*Ferrussac's Bulletin.*

Preparation of Pure Potash.—Mr Donovan proposes the following as a more easy method of obtaining pure potash than the methods commonly employed.

The crystallized bicarbonate of potash of the shops is to be purified by dissolving it in water at the temperature of 100°. The saturated solution must be filtered and poured into a flat dish, and placed before the fire; in a few hours a crop of crystals of the pure bicarbonate will be obtained. The crystals may then be rinsed with a very small quantity of water, and dried on blotting paper.

The crystals are now to be dissolved in water, and boiled with their own weight of hydrate of lime for 15 minutes; the solution is then to be filtered in the usual manner. We have thus at once a solution of pure potash, without the additional trouble of evaporation and solution in alcohol; and we obviate the reconversion of the alkali into carbonate, which always happens during the evaporation in the common process, unless the tedious and troublesome method of evaporating without contact of the atmosphere be adopted, in which case silver vessels become necessary.

As a test to ascertain whether or not a solution of potash be perfectly caustic, chemists make use of a dilute acid; but this method gives no information unless the acid be added in excess. A small quantity will only displace the carbonic acid from one portion of potash; but the remaining portion will unite with the liberated acid so as to prevent any appearance of effervescence. Thus an alkali that is in fact partly carbonated will not be affected apparently by the affusion of a small quantity of a test acid.—*Dublin Philosophical Journal.*

New Project for a Transporting Apparatus.—Among the numerous projects for improving the methods of transportation, started by the increased interest that the public has lately manifested in rail roads, an invention by Mr Vallance of Brighton, in England, deserves notice, for its strangeness, if not for its ingenuity. It has been called in some journals a mode of "travelling in vacuo." The apparatus consists, essentially, of an air tight tunnel, to be formed from one town to another, about 11 feet in diameter; upon the inside of this a rail way is to be formed, and the carriages adapted to it are to be furnished with a plane, which, like a throttle valve, fills up nearly the whole cross section of the tunnel. It will readily be perceived, that on partially exhausting the air at the end of the tunnel towards which it is intended the carriage shall move; the pressure of the air from the opposite end, acting on the plane fixed to the carriage, will cause it to advance with a velocity corresponding to the degree to which the exhaustion is carried. It is intended to produce the exhaustion by pumps worked, of course, by the steam engine.

Nature of Colour in Mineral Productions.—A correspondent in the Annals of Philosophy was led to doubt of the constant necessity of any specifically colouring ingredient in minerals, from observing that crystallized feldspar exhibits a decided red colour, though analysis points out no substance to which it could be owing; and also from observing that the mineral, after exposure to a strong heat, has the colour entirely destroyed, a colourless glass being obtained. By accident he obtained an artificial mixture of bodies, which exhibited a property of this kind, becoming coloured by particular treatment, though containing nothing to which that colour could be particularly referred. Lime, alumina, silica, soda, and boracic acid, were coarsely mixed together, and exposed to a strong white heat, which produced a semi-vitrified mass of a pure white; a portion of this was finely ground, and after exposure to a low red heat, not above that of melting silver, was found, with surprise, to have assumed a red colour; which colour, with the increase of heat, entirely disappeared, and the substance assumed, at last, as pure a white as it possessed after the first fusion. A lump of the original mass underwent no changes of colour at the same heat, and it was uniformly found, after several trials, that the depth of the colour depended on the fineness of grinding. Nitre destroyed the colour in the fire.

Specimens were sent to the editor, according to whom the pulverised portion, exposed to a low red heat, was of a peach-blossom colour; another portion, exposed to a higher temperature, pale bluish lilac; a third portion, exposed to a moderate white heat, was a slightly greenish white enamel; and a fourth portion, mixed and heated with one fifth nitre, was like the second of the above.—*Ann. Philos.* N.S. IX. 432.

Method of making Transparent Soap.—Tallow is the basis of all soaps for the toilette, known under the name of Windsor, because olive oil forms a paste too difficult to melt, and having an odour too powerful for mixing with perfumes.

Tallow-soap, dissolved with heat in alcohol, returns to its solid state on cooling. It is this fact which has led to the discovery of transparent soap. When well prepared, this soap should have the appearance of fine white sugar-candy. It may also be coloured, and vegetable colours are, for this purpose, preferable to minerals. Any person can make this soap by putting into a thin glass phial half a brick of Windsor soap, cut small, filling the phial half full of alcohol, and placing it near the fire till the soap is dissolved. This mixture, put to cool in a mould, gives the transparent soap.—*Archives des Découvertes et des Inventions Nouvelles.*

On the Employment of the Wood and Bark of the Chestnut-tree in Dyeing and Tanning.—The bark of the chestnut-tree contains twice as much tanning matter as oak-bark, and nearly twice as much colouring matter as logwood. The colouring substance of chestnut-bark is to that of Campeachy logwood exactly as 1.857 to 1.

Leather prepared with this substance is more firm and solid, and yet more supple. This bark is the best substance for making ink; mixed with iron, it becomes a bluish black. The liquor drawn from this bark appears blue at the outside, like indigo; but it gives, on paper, the finest black. In dyeing it has a greater affinity for wool than sumach has, and in other respects it differs but little from sumach and gall-nuts. The colour obtained from this substance is unchangeable by air and light.—*Annales de l'Industrie National.*

Condensation of a Mixture of Hydrogen and Oxygen by pulverulent Platinum.—Dœbereiner has ascertained that *moist* as well as *dry* platinum causes the mutual condensation of these gases. The effect in both cases is equally complete; the only difference being in the length of time necessary to produce it.

The best method of performing the experiment, is to ignite at the bottom of a glass tube, closed by fusion at one extremity, a quantity of the double ammonio-muriate of platinum, or to decompose in it a solution of platinum by means of a rod of zinc. In either case, a thin film of platinum is deposited upon the interior of the tube, and adheres with considerable firmness. If a tube thus prepared be filled with a mixture of hydrogen and oxygen (or atmospheric air) and inverted over water, the whole of the hydrogen will be condensed into water in the course of a few hours. A similar result is obtained by placing a mass of spongy platinum well soaked with water into a receiver filled with the mixture of the two gases. He next examined what would be the effect of moistening the platinum with other liquids. With alcohol the experiment succeeded equally as well as with water; but not the slightest condensation took place when the spongy metal was imbibed with acid, or with liquid ammonia. He ascribes these differences exclusively to the gaseous mixture being absorbable by water and alcohol, but not by nitric acid or liquid ammonia; in the former case only, the gases would be conveyed into immediate contact with the metal. Dœbereiner concludes with observing, that the existence of some peculiar and independent property in the platinum is more decisively evinced by the present experiment than by any other which he had heretofore made.

These experiments suggested an easy method of depurating hydrogen from minute traces of oxygen. All that is necessary is to enclose it in a stoppered phial, a portion of whose interior has been coated by the process just described, with a thin incrustation of platinum. The oxygen will by degrees undergo condensation.—*Schweigger's Neues Jornal für Chemie und Physik*, xii. 60.

On the Means of testing for Iodine.—Notwithstanding the delicacy of the test for the presence of iodine afforded by the use of starch, its value is considerably diminished by the facility with which various substances interfere with its action; and this is especially the case when any of those bodies, which, either alone or with water, yield hydrogen, are present; the iodine becomes hydriodic acid, and the blue colour either disappears, or is not formed. Sulphurous acid and sulphuretted hydrogen, substances almost constantly produced by the incineration of organized bodies which contain earthy or alkaline sulphates, are especially capable of producing this effect.

Among the various means of obviating this inconvenience is the use of chlorine, and during his experiments on the products of the Mediterranean, M. Baland has been led to apply it in the following manner:—after having mixed the liquid containing the iodine with the starch and the sulphuric acid, a small quantity of aqueous solution of chlorine is to be added, which from its lightness may be made not to mix with the mixture, but float on the surface; at the place, however, where they touch, a blue zone will be developed where the two solutions are in contact, but if the whole be mixed, it will entirely disappear, if the chlorine be in excess.

By means of this test thus applied, M. Baland has found iodine where it could not before be observed, as in the various marine molluscæ, testaceous or not, such as the *doris*, *venus*, *oysters*, &c. many polypi and marine vegetables, the *gorgonia*, the *zostera marina*, &c. and especially in the mother water of the salt pans fed by the Mediterranean. The small quantity of iodine which is found in sea water, he believes, from the mode of action of the test, to exist as a hydriodate.

M. Baland expresses his conviction that a process of this kind will save chemists the trouble of many fruitless trials, and lead to decided and positive results, where otherwise none would be obtained.—*Ann. de Chimie.*

Anthracite of Pennsylvania.—Mr Maclure. in a letter to Prof. Silliman, remarks, that, this immense formation of anthracite will render this state the most productive in the union; for as soon as they can contrive a good mode of smelting iron ore with the anthracite, this will become one of the greatest iron countries on the globe, because, having so much fine magnetic iron ore, and the natural state of the combustible rendering it capable of producing a very strong heat, without any preparation of coaking or (being) adulterated with any mixture injurious to the making of iron; these circumstances constitute so many advantages as are scarcely to be met with in any one locality as yet known. The benefit that will arise to the nation from a proper method of smelting iron with anthracite, is such an object as to interest the ingenuity of all classes. For the purpose of experiment, it is necessary to procure a furnace on a small scale, as the high furance is too expensive. The fault complained of being that the mass freezes, or does not retain its fluidity sufficiently, perhaps from the blast being too weak, or too cold or not sufficiently disseminated through the mass; owing, perhaps, to the want of the proper proportions of ore, coal, and flux; the form of the furnace used for charcoal may not be fit for a much greater heat, &c. In short, it may be necessary to try all proportions of blast, fuel, ore, and flux, with all forms and dimensions of furnace.—*Amer. Jour.* No. 21.

Comet of 1825.—We are indebted to R. T. Paine, Esq. for the following account of the late comet.

"When I first saw the comet, it was in the constellation Taurus, and so near the Hyades, that its tail swept over them. In its path it passed from Taurus into Cetus, Eridanus, Cetus again, Fornax Chemica, Apparatus Sculptoris and Phœnix, in which last constellation it was seen for the last time, on Monday evening, Oct. 17th. The following distances it is hoped may be of some small use for the purpose of comparison, or perhaps they may be employed in connection with other observations.

Distance of the comet from (at 10h. 30m. P. M.)

	Oct. 5th.	Oct. 6th.	Oct. 7th.	Oct. 12th.	Oct. 16th.
α piscis aus.	56° 03′	48° 17′	45° 23′	31° 18′	21° 56′
α Androm.	51 32	57 02	57 41		
β. Ceti				14 45	19 41
A. R.	36 15	34 01	31 46	17 45	6 30
N. P. D.	106 47	108 46	111 10	120 30	128 00

Celestial Phenomena for November and December, 1825, *for Cambridge, in latitude* 42° 23′ 28″ *N. long.* 71° 7′ 25″ *W.* Mean time. (Communicated by R. T. Paine, Esq.)

	D.	H.	M.	S.	
Nov.	2	1	33	15	M. Im. 1st Sat. ♃.
—	3	1	18	08	A. ☌ ☽ 2 a ♋.
—	3	0	8	28	A. ☌ ☽ 1 a ♋.
—	9	2	32	46	M. Em. 3d. Sat. ♃.
—	9	3	26	48	M. Im. 1st. Sat. ♃.
—	12	0	45	30	M. Im. 2d. Sat. ♃.
—	16	2	7	46	M. Em. 4th. Sat. ♃.
—	16	3	2	41	M. Im. 3d. Sat. ♃.
—	16	5	20	16	M. Im. 1st. Sat. ♃.
—	19	3	18	53	M. Im. 2d. Sat. ♃.
—	22	8	18	08	M. ☉ enters ♐.
—	24	6	17	16	M. ☌ ☽ δ ♈.

	D.	H.	M.	S.	
Nov.	25	1	42	05	M. Im. 1st. Sat. ♃.
—	25				☽ eclipsed invisible.
—	26	5	52	21	M. Im. 2d. Sat. ♃.
—	26				Occultation of ♄ by ☽.
—	"	7	8	55	A. ☌ in Eliptic.
—	"	5	46	18	A. Im. of ♄ W. Limb, 13′, 2 S of ☽′, C.
—	"	5	41	35	A. Im. of ♄ E. Limb.
—	"	6	9	51	A. Em. of ♄ W. Limb 13′, 5 S of ☽ C.
—	"	6	11	08	A. Em. of ♄ E. Limb.
—	27	11	19	14	A. ☌ ☾ ν. ♊.

	D.	H.	M.	S.	
Dec.	2	3	35	27	M. Im. 1st. Sat. ♃.
—	8	10	35	40	M. ☌ ☽ ♂ ♏.
—	9	5	28	47	M. Im. 1st. Sat. ♃.
—	9				☉ eclipsed, partly visible.
—	9	3	42	40,1	A. ☌ of ☉ and ☽ in ecliptic.
—	9	3	52	58,2	A eclipse begins.
—	9	4	22	20	A. ☉ sets eclipsed.
—	10	11	57	05	A. Im. Sat. ♃.
—	14	0	16	41	M. Im. 2d. Sat. ♃.
—	18	1	50	23	M. Im. 1st. Sat. ♃.
—	21	2	50	41	M. Im. 2d. Sat. ♃.
—	21	9	01		A. ☉ ent. ♑ winter begins.
—	22	11	57	32	A. ☌ ☽ χ ♉, a near approach.
—	22	2	16	13	M. Em. 3d. Sat. ♃.
—	23	4	45	37	A. ☌ ☽ ι ♉.
—	23	10	17		A. ☌ ☽ ♄.
—	25	3	43	41	M. Im. 1st. Sat. ♃.
—	25	6	17	36	M. ☌ ☽ ν ♊.
—	27	11	37	02	A. ☌ ☽ 1 α ♋.
—	27	11	54	04	A.Im.of Acubens 14′,5 } s.of ☽′
—	28	0	32	49	M. Em. of do. 12,7 } centre.
—	28	5	24	48	M, Im. 2d. Sat. ♃.
—	29	2	48	58	M. Im. 3d. Sat. ♃.
—	29	6	13	30	M. Em. 3d. Sat. ♃.

(*To be continued.*)

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* The moon will first touch the sun 155° 1′ from the highest point of his disc at sunset; 4° 51′ will be eclipsed. At New Orleans 9° 39′ will be eclipsed, at St Augustine 10° 52′ on the south, and at cape Florida 11° 32 on the north limb. The path of the central and total eclipse will strike the United States, near cape Roman on the western, and will leave them near cape East, on the eastern

Additions to the Cabinet of Minerals at Cambridge.

	Locality.	*Presented by*
Pimelite	New Fane	Gen. Field.
Chrysoprase		
Actynolite		
Zoisite		
Chiastolite	Lancaster	Mr F. Plummer.
Sienite, being a portion of "Forefather's Rock"	Plymouth	Mr Spooner.
Chloropheite	Turner's Falls, Gill	Mr Hitchcock.
Fossil oyster	W. Indies	Miss Langdon.
Peruvian Minerals	S. America	Capt. Smith.
Lepidolite	Paris, Maine	J. W. Webster.
Beryls		
Rock crystals		
Red and Green Tourmaline		
Actynolite		
Mica		
Cleavelandite		
Pseudomorphous steatite		
Rose Quartz, &c.		
Scapolite	Bolton and Littleton Mass.	
Spinel		
Pyrallolite		
Petalite, &c.		
Beryls in Quartz	Topsham and Bowdoinham, Me.	
Green Mica		
Tourmaline		
Rose Quartz		
Graphic Granite, &c.		

Botanic Garden, Cambridge.

A list of the rarer and native plants which have flowered in the Botanic Garden at Cambridge during the last two months. [Communicated by Mr Nuttall.]

Aster novæ-angliæ, cyaneus, undulatus, cordifolius, concolor, carolinianus, amygdalinus, puniceus, surculosus, linarifolius, graveolens, multiflorus, sibiricus, chinensis.

Amaryllis Belladonna, falcata, undulata, stellaris.

Acacia pulchella, farnesiana, lophantha.

Baccharis halimifolia.

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side of East Florida. This will be the last eclipse of the sun that will be visible at Cambridge, until the remarkable eclipse of February 12th 1831, which eclipse will be annular at cape Charles in the state of Virginia, at the island of Nantucket in this state, and at Halifax, in the province of Nova Scotia. The next total eclipse of the sun visible in the United states, will happen November 30th 1834, when the sun will be totally eclipsed at the cities of Charleston and Beaufort, in the state of South Carolina.

Campanula pyramidalis.
Casssuarina equisetifolia.
Chelone campanulata.
Cyclamen coum.
Chrysanthemum indicum, (many varieties.)
Dahlia pinnata, (many varieties.)
Diosma ciliaris.
Dracocephalum peregrinum, virginicum, denticulatum, moldavicum.
Erica Plukenetii, triceps, tenella, hirta, spicata, mammosa, transparens.
Epacris juniperina.
Euphorbia neapolitana, meloformis.
Eriogonum longifolium.
Globba marantina.
Hedychium coccineum, maximum, flavum, longifolium, thyrsiforme, fastigiatum, gardenerianum.
Hibiscus syriacus, virginicus, palustris, moscheutos.
Helenium autumnale.
Hioseris radiata.
Ligusticum scoticum.
Melastoma, (from Nepal.)
Mespilus japonica.
Mimulus luteus.
Monarda punctata.
Nepeta nepetella, augustifolia, incana.
Olea fragrans, capensis.
Oxalis versicolor, rubella, rosacea.
Œnothera serrulata.
Pelargonium acetosum, peltatum.
Phylica ericoides.
Phlomis leonurus.
Physalis somnifera, peruviana.
Protea mellifera.
Royena cuneata.
Sesbania macrocarpa.
Silene pendula.
Solanum sanctum, fontanesianum.
Solidago angustifolia, puberula, scabra, altissima, odora, nemoralis, lanceolata, bicolor, cæsia, maritima.
Thea Bohea, viridis.
Veronica australis.
Vitex agnus castus.
Volkameria fragrans.
Yucca gloriosa.
Zimenesia encelioides.
Zinziber chyrysanthum.

ERRATA.

Vol. 2, p. 609, ninth line from top, for 2.39 read 3.2.
" " twentieth line, for *pliable* read *friable*.

Art. X.—*Biographical Account of M. le Comte Legrange.* By M. le Chevalier Delambre. [*Ann. Philos.*]

Joseph-Louis Lagrange, one of the founders of the Academy of Turin, Director during 20 years of the Berlin Academy for the Physico-Mathematical Sciences, Foreign Associate of the Academy of Sciences of Paris, Member of the Imperial Institute and of the Board of Longitude, Senator and Comte of the Empire, Grand Officer of the Legion of Honour, Grand Cross of the Imperial Order of Re-union, was born at Turin, on the 25th of November, 1736. He was the son of Joseph-Louis Lagrange, Treasurer of War, and of Marie-Therese Gros, only daughter of a rich physician of Cambiano. His great-grandfather, a captain of horse in the service of France, had gone over to that of Emanuel II. King of Sardinia, who established him at Turin by marrying him to a lady of Conti, of an illustrious Roman family. He was originally a Parisian, and a relation of Marie-Louise, dressing maid to the mother of Louis XIV., and afterwards wife of Francis Gaston de Bethune.

These details are of no importance for the illustrious mathematician, whose reputation renders his genealogy of no consequence to him; but they are not indifferent to France, who eagerly recalled him, and restored him to his ancient rights. His name, and that of his mother, show that he was of French extraction. All his works were written in French. The city in which he was born has become a part of the French empire. France therefore has indisputably the right of boasting of one of the greatest geniuses that have done honor to the sciences.

His father was rich, and had made an advantageous marriage; but was ruined by hazardous undertakings. Let us not however lament the situation of M. Lagrange. He himself viewed it as the first cause of all the good fortune that afterwards befell him. "Had I been in possession of a fortune," said he, "I should not probably have studied mathematics." In what other situation would he have found advantages that could enter into comparison with those of a tranquil and studious life, with that splendid series of discoveries in a branch of science considered as the most difficult, and with that personal respectability which was continually increasing to the very last period of his life.

His taste for mathematics did not appear at first. He was passionately devoted to Cicero and Virgil, before he could read Archimedes and Newton. He then became an enthusiastic admirer of the geometry of the ancients, which he preferred to the modern analysis. A memoir which the celebrated Halley had composed long before, to demonstrate the superiority of the analytic method, had the glory of converting him, and teaching him his true destiny. He devoted himself to this new study with the same success that he had had in the synthesis, and which was so decided that at the age of 16 he was Professor of Mathematics in the Royal Military School. The extreme youth of a Professor is a great advantage to him when he has shown extraordinary abilities and when his pupils are no longer children. All the pupils of M. Lagrange were older than himself, and were not the less attentive to his lectures on that account. He distinguished some of them whom he made his friends.

From this association sprung the Academy of Turin, which in 1759 published a first volume, entitled Acts of a private Society. We see there the young Lagrange directing the philosophical researches of the physician Cigna, and the labours of the Chevalier de Saluces. He furnished to Foncenex the analytical part of his memoirs, leaving to him the task of developing the reasoning upon which the formulas depended. In these memoirs, which do not bear his name, we observe that purely analytical method, which afterwards characterized his great productions. He had found a new theory of the lever. It constitutes the third part of a memoir, which was very successful. Foncenex, in recompense, was placed at the head of the Marine, which the king of Sardinia formed at that time. The two first parts have the same style and seem written by the same person. Do they likewise belong to Lagrange? He never expressly laid claim to them; but what may give us some light into the real author, is that Foncenex soon ceased to enrich the volumes of the new Academy, and that Montucla, ignorant of what Lagrange revealed to us during the latter part of his life, is astonished that Foncenex interrupted those researches which might have given him a great reputation.

M. Lagrange, while he abandoned to his friend insulated theorems, published at the same time, under his own name, theories which he promised to develope further. Thus after having given new formulas of the *maximum* and *minimum* in all cases, after having shown the insufficiency of the known methods, he announces that he will treat this subject, which he considered as important, in a work which he was preparing, in which would be deduced from the same principles all the mechanical properties of bodies, whether solid or fluid. Thus at the age of 23 he had laid the foundation of the great works, which constitute the admiration of philosophers.

In the same volume he reduces under the differential calculus the theory of recurrent series and the doctrine of chances; which before that time had only been treated by indirect methods. He established them upon more natural and general principles.

Newton had undertaken to submit the motions of fluids to calculation. He had made researches on the propagation of sound; but his principles were insufficient and even faulty, and his suppositions inconsistent with each other. Lagrange demonstrates this. He founds his new researches on the known laws of dynamics, and by considering only in the air the particles which are in a straight line, he reduces the problem to that of vibrating cords, respecting which the greatest mathematicians differed in opinion. He shows that their calculations are insufficient to decide the question. He undertakes a general solution by an analysis equally new and interesting, which enables him to resolve at once an indefinite number of equations, and which embraces even discontinued functions. He establishes on more solid grounds the theory of the mixture of simple and regular vibrations of Daniel Bernoulli. He shows the limits within which this theory is exact, and beyond which it becomes faulty. Then he comes to the construction given by Euler, a construction true in itself, although its first author had arrived at it by calculations which were not quite rigorous. He answers the objections of D'Alembert. He demonstrates that whatever figure is given to the cord, the duration of the oscillations is always the same; a truth derived from experiment which D'Alembert considered as very difficult if not impossible to demonstrate. He passes to the propagation of sound, treats of simple and compound echos, of the mixture of sounds, of the possibility of their spreading in the same space without interfering with each other. He demonstrates rigorously the generation of harmonious sounds. Finally, he announces that his intention is to destroy the prejudices of those who still doubt whether the mathematics can ever throw a real light upon physics.

We have given this long account of that memoir, because it is the first by which M. Lagrange became known. If the analytical reasoning in it be of the most transcendent kind, the object at least has something sensible. He recalls names and facts which are well known to most people. What is surprising is, that such a first essay should be the production of a young man, who took possession of a subject treated by Newton, Taylor, Bernoulli, D'Alembert, and Euler. He appears all at once in the midst of these great mathematicians as their equal, as a judge, who in order to put an end to a difficult dispute, points out how far each of them is in the right, and how far they have deceived themselves; determines the dispute between them, corrects their errors, and gives them the true solution, which they had perceived without knowing it to be so.

Euler saw the merit of the new method, and took it for the object of his profoundest meditations. D'Alembert did not yield the point in dispute. In his private letters, as well as in his printed memoirs, he proposed numerous objections, to which Lagrange afterwards answered. But these objections may give rise to this question: How comes it that, in a science in which every one admits the merit of exactness, geniuses of the first order take different sides, and continue to dispute for a long time? The reason is, that in problems of this kind, the solutions of which cannot be subjected to the proof of experiment, besides the part of the calculation which is subjected to rigorous laws, and respecting which it is not possible to entertain two opinions, there is always a metaphysical part which leaves doubt and obscurity. It is because in the calculations themselves mathematicians are often content with pointing out the way in which the demonstration may be made; they suppress the developements which are not always so superfluous as they think. The care of filling up these blanks would require a labour which the author alone would have the courage to accomplish. Even he himself, drawn on by his subject and by the habits which he has acquired, allows himself to leap over the intermediate ideas. He divines his definite equation, instead of arriving at it step by step with an attention that would prevent every mistake. Hence it happens that more timid calculators sometimes point out mistakes in the calculations of an Euler, a D'Alembert, a Lagrange. Hence it happens that men of very great genius do not at first agree, from not having studied each other with sufficient attention to understand each other's meaning.

The first answer of Euler was to make Lagrange an associate of the Berlin Academy. When he announced to him this nomination on the 20th of October, 1759, he said, "Your solution of the problem of isoperimetres leaves nothing to desire; and I am happy that this subject, with which I was almost alone occupied since the first attempts, has been carried by you to the highest degree of perfection. The importance of the matter has induced me to draw up, with your assistance, an analytical solution of it. But I shall not publish it, till you yourself have published the sequel of your researches, that I may not deprive you of any part of the glory which is your due."

If these delicate proceedings and the testimonies of the highest esteem were very flattering to a young man of 24 years of age, they do no less honour to the great man, who at that time swayed the sceptre of mathematics, and who thus accurately estimated the merit of a work that announced to him a successor.

But these praises are to be found in a letter. It may be supposed that the great and good Euler has indulged in some of those exaggerations which the epistolary style permits. Let us see then how he has expressed himself in the dissertation which his letter announced. It begins as follows:

"After having fatigued myself for a long time and to no purpose in endeavouring to find this integral, what was my astonishment when I learnt that in the Turin Memoirs the problem was resolved with as much facility as felicity. This fine discovery produced in me so much the more admiration, as it is very different from the methods which I have given, and far surpasses them all in simplicity."

It is thus that Euler begins the memoir in which he explains with his usual clearness the foundation of the method of his young rival, and the theory of the new calculus, which he called the *calculus of variations*.

To make the motives of this admiration which Euler bestowed with so much frankness better understood, it will not be useless to go back to the origin of the researches of Lagrange, such as he stated them himself two days before his death.

The first attempts to determine the *maximum* and *minimum* in all indefinite integral formulas, were made upon the occasion of the curve of swiftest descent, and the isoperimetres of Bernoulli. Euler had brought them to a general method, in an original work, in which the profoundest knowledge of the calculus is conspicuous. But however ingenious his method was, it had not all the simplicity which one would wish to see in a work of pure analysis. The author admitted this himself. He allowed the necessity of a demonstration independent of geometry. He appeared to doubt the resources of analysis, and terminated his work by saying, "If my principle be not sufficiently demonstrated, yet as it is conformable to truth, I have no doubt that by means of a rigid metaphysical explanation it may be put in the clearest light, and I leave that task to the metaphysicians."

This appeal, to which the metaphysicians paid no attention, was listened to by Lagrange, and excited his emulation. In a short time the young man found the solution of which Euler had despaired. *He found it by analysis.* And in giving an account of the way in which he had been led to that discovery, he said expressly, and as it were in answer to Euler's doubt, that he regarded it not as a metaphysical principle, but as a necessary result of the laws of mechanics, as a simple corollary from a more general law, which he afterwards made the foundation of his Mechanique Analytique. (See that work, page 189 of the first edition.)

This noble emulation which excited him to triumph over difficulties considered as insurmountable, and to rectify or complete theories remaining imperfect, appears to have always directed M. Lagrange in the choice of his subjects.

D'Alembert had considered it as impossible to subject to calculations the motions of a fluid inclosed in a vessel, unless this vessel had a certain figure. Lagrange demonstrates the contrary; except in the case when the fluid divides itself into different masses. But even then we may determine the places where the fluid divides itself into different portions, and determine the motion of each as if it were alone.

D'Alembert had thought that in a fluid mass, such as the earth may have been at its origin, it was necessary for the different beds to be on a level. Lagrange shows that the equations of D'Alembert are themselves equations of beds on a level.

In combating D'Alembert with all the delicacy due to a mathematician of his rank, he often employs very beautiful theorems, for which he was indebted to his adversary. D'Alembert on his side added to the researches of Lagrange. "Your problem appeared to me so beautiful," says he in a letter to Lagrange, "that I have sought for another solution of it. I have found a simpler method of arriving at your elegant formula." These examples, which it would be easy to multiply, prove with what politeness these celebrated rivals corresponded, who, opposing each other without intermission, whether conquerors or conquered, constantly found in their discussion reasons for esteeming each other more, and furnished to their antagonist occasions which might lead them to new triumphs.

The Academy of Sciences of Paris had proposed, as the subject of a prize, the theory of the libration of the moon. That is to say, they demanded the cause why the moon, in revolving round the earth, always turns the same face to it, some variations excepted, observed by astronomers, and of which Cassini had first explained the mechanism. The point was to calculate all the phenomena, and to deduce them from the principle of universal gravitation. Such a subject was an appeal to the genius of Lagrange, an opportunity furnished to apply his analytical principles and discoveries. The attempt of D'Alembert was not disappointed. The memoir of Lagrange is one of his finest pieces. We see in it the first developement of his ideas, and the germ of his Mechanique Analytique. D'Alembert wrote to him: "I have read with as much pleasure as advantage your excellent paper on the Libration, so worthy of the prize which it obtained."

This success encouraged the Academy to propose, as a prize, the theory of the satellites of Jupiter. Euler, Clairaut, and D'Alembert had employed themselves about the problem of three bodies on occasion of the movements of the moon. Bailly then applied the theory of Clairaut to the problem of the satellites, and it had led him to very interesting results. But this theory was insufficient. The earth has only one moon while Jupiter has four, which ought continually to act upon each other and alter their positions in their revolutions. The problem was that of six bodies. Lagrange attacked the difficulty and overcame it, demonstrated the cause of the inequalities observed by astronomers, and pointed out some others too feeble to be ascertained by observations. The shortness of the time allowed, and the immensity of the calculations, both analytical and numerical, did not permit him to exhaust the subject entirely in a first memoir. He was sensible of this himself, and promised further results, which his other labours always prevented him from giving. Twenty-four years after M. Laplace took up that difficult theory, and made important discoveries in it, which completed it and put it in the power of astronomers to banish empiricism from their tables.

About the same time a problem of quite a different kind drew the attention of M. Lagrange. Fermat, one of the greatest mathematicians of his time, had left very remarkable theorems respecting the properties of numbers, which he probably discovered by induction. He had promised the demonstrations of them; but at his death no trace of them could be found. Whether he had suppressed them as insufficient, or from some other cause, cannot now be ascertained. These theorems perhaps may appear more curious than useful. But it is well known that difficulty constitutes a strong attraction for men, especially mathematicians. Without such a motive would they have attached so much importance to the problems of the brachytochrone, of the isoperimetres, and of the orthogonal trajectories? Certainly not. They wished to create the science of calculation, and to perfect methods which could not fail some day of finding useful applications. With this view they attached themselves to the first question which required new resources. The system of the world discovered by Newton was a most fortunate event for them. Never could the transcendental calculus find a subject more worthy or more rich. Whatever progress is made in it, the first discoverer will always retain his rank. Accordingly, M. Lagrange, who cites him often as the greatest genius that ever existed, adds also, "and the most fortunate. We do not find every day a system of the world to establish." It has required 100 years of labours and discoveries to raise the edifice of which Newton laid the foundation. But every thing is ascribed to him, and we suppose him to have traversed the whole country upon which he merely entered.

Many mathematicians doubtless employed themselves on the theorems of Fermat; but none have been successful. Euler alone had penetrated into that difficult road in which M. Legendre and M. Gauss afterwards signalized themselves. M. Lagrange in demonstrating or rectifying some opinions of Euler, resolved a problem which appears to be the key of all the others; and from which he deduced a useful result; namely, the complete resolution of equations of the second degree, with two indeterminates which must be whole numbers.

This memoir, printed like the preceding, among those of the Turin Academy, is notwithstanding dated Berlin, the 20th September, 1768. This date points out to us one of the few events which render the life of Lagrange not entirely a detail of his writings.

His stay at Turin was not agreeable to him. He saw no person there who cultivated the mathematics with success. He was impatient to see the philosophers of Paris with whom he corresponded. M. de Caraccioli, with whom he lived in the greatest intimacy, was appointed ambassador to London, and was to pass through Paris on his way, where he intended to spend some time. He proposed this journey to M. Lagrange, who consented to it with joy, and who was received as he had a right to expect by D'Alembert, Clairaut, Condorcet, Fontaine, Nollet, Marie, and the other philosophers. Falling dangerously ill after a dinner in the Italian style given him by Nollet, he was not able to accompany his friend to London, who had received sudden orders to repair to his post, and who was obliged to leave him in a furnished lodging under the care of a confident charged to provide every thing.

This incident changed his projects. He thought only of returning to Turin. He devoted himself to the mathematics with new ardour, when he understood that the Academy of Berlin was threatened with the loss of Euler, who thought of returning to Petersburgh. D'Alembert speaks of this project of Euler, in a letter to Voltaire, dated the 3d of March, 1766: "I shall be sorry for it," says he, "he is a man by no means amusing, but a very great mathematician." It was of little consequence to D'Alembert whether this man *by no means amusing*, went 7 degrees nearer the pole. He could read the works of the great mathematician as well in the Petersburgh Memoirs as in those of Berlin. What troubled D'Alembert was the fear of seeing himself called upon to fill his place, and the difficulty of giving an answer to offers which he was determined not to accept. Frederick in fact offered him again the place of President of his Academy, which he had kept in reserve for him ever since the death of Maupertuis. D'Alembert suggested the idea of putting Lagrange in the place of Euler, and, if we believe the author of the Secret History of the Court of Berlin, (vol. ii. p. 414) Euler had already pointed out Lagrange as the only man capable of filling his place. In fact it was natural that Euler, who wished to obtain permission to leave Berlin, and D'Alembert who wanted a pretext not to go there, should both of them without any communication, have cast their eyes on the man who was best fitted to maintain the eclat which the labours of Euler had thrown round the Berlin Academy.

Lagrange was pitched upon. He received a pension of 1500 Prussian crowns, about 250*l.*, with the title of Director of the Academy for the Physico mathematical Sciences. We may be surprised that Euler and Lagrange, put successively in the place of Maupertius, received only the half of his salary, which the king offered entire to D'Alembert. The reason is, that this prince, who at his leisure hours cultivated poetry and the arts, had no idea of the sciences, though he considered himself obliged to protect them as a king. He had very little respect for the mathematics, against which he wrote three pages in verse, and sent them to D'Alembert himself, who deferred writing an answer till the termination of the siege of Schweidnitz; because he thought it would be too much to have both Austria and the mathematics on his hands at once. Notwithstanding the prodigious reputation of Euler, we see, from the king's correspondence with Voltaire, that he gave him no other appellation than his *narrow-minded geometer, whose ears were not capable of feeling the delicacy of poetry*. To which Voltaire replies: *We are a small number of adepts who know one another; the rest are profane.* We see well that Voltaire who had written so well in praise of Newton, endeavours in this place to flatter Frederick. He enters out of complaisance into the ideas of this prince, who wished to put at the head of his academy, a man who had at least some pretensions to literature. Fearing that a mathematician would not take sufficient interest in the direction of literary labours; and that a man of literature would have been still worse placed at the head of a society composed in part of philosophers of whose language he was ignorant; on that account he divided the situation, and put two persons in it, that it might be completely filled.

[*To be continued.*]

ART. XI.—*On the Mode of obtaining Silicium, and on the Characters and Properties of that Substance.* By M. Berzelius.

(Concluded from page 45.)

Preparation of Silicium.

THE combustion of potassium in silicated fluoric gas requires apparatus which we have not always at hand. The double salts, on the contrary, which fluoric acid forms with silica and potash, or soda, afford very advantageous means of preparing silicium. I did not observe any difference in the use of one or the other of these salts. That of soda has however the advantage of containing under the same weight and the same volume a greater quantity of the fluate of silica. This salt is used in the following manner:—It is reduced to a fine powder; and in case the desiccation has made its particles adhere, and also to expel the humidity that may remain, it is exposed to a temperature as high as it can bear without being decomposed, that is to say, much above 100°. It is then put by layers with the potassium into a tube of glass closed at one end, which is placed in such a manner as that all the mass may be heated at the same time. We may, if we choose, melt the potassium and mix it, by means of a rod of iron, with the salt, and then heat the mass over a spirit-lamp. Even before a red heat, the silicium is reduced with a slight hissing and a feeble appearance of heat. No gas is disengaged when the salt has been well dried. The mass is left to cool, and then treated as before described. It ought however to be dissolved in a great quantity of water; so that the liquid, which becomes alkaline by the oxidation of the potassium, may be as diluted as possible, and have less tendency to oxidate and dissolve the silicium. It is for this reason that the mass ought not to be treated with hot water,—that the liquid has not lost its alkaline quality by the washing. It is afterwards boiled with water, and washed with hot water until no stain is left by the evaporation of a drop of liquid. For this purpose much time is generally requisite, and much water must be used. The silicium obtained by this process contains hydrogen; but in a smaller quantity, however, and perhaps in the same manner as the common charcoal of wood, which Davy considers as hydrogenated carbon. It contains also silica, which proceeds principally from the potassium before the reduction, being oxidated a little, and then separating a quantity of silica corresponding to the alkali formed; but the alkali which is formed after the reduction, when it dissolves the mass in the water, dissolves a portion of the double salt in excess, without separating the silica from it. This silica ought to be taken away by means of fluoric acid; but as the silicium would dissolve in the acid, we must begin by rendering it insoluble and incombustible; if it were made to burn in air, a portion of incombustible silicium would indeed be obtained after treatment by fluoric acid; but ordinarily two thirds of it would be lost by the combustion. This loss is prevented by heating nearly to ignition the dried silicium containing hydrogen in an open crucible; this degree of heat is maintained some time, and then it is raised little by little to a perfect red heat. If the silicium become inflamed, the crucible should be covered directly, and the temperature lowered, which will instantly stop the combustion. When the calcination is finished, the silicium is incombustible in air, and is no longer attacked by fluoric acid, if it does not contain any foreign metal, iron, or manganese for example; for in this case the alloy would be entirely dissolved with disengagement of hydrogen. After treatment by the acid, the silicium is washed and dried. It might be thought that this incombustibility is caused by an extremely thin pellicle of silica; but I dried the silicium in a vacuum, and then heated it to redness in the air, and I did not find any change of weight.

The silica may be reduced by heating it with potassium: but it happens either that the combination, rich in potassium, becomes entirely dissolved in the water; or else, when the heat is sufficient to expel the excess of potassium, that the silicate of potash formed melts into a vitreous mass, and envelops the silicium, which thence acquires a clearer colour. Part of the silicate may be separated by means of water, but the remainder can only be removed by fluoric acid. The quantity of silicium is extremely small; and this manner of obtaining it merits attention only because it gives the same result as the double salt when treated with potassium; which proves that the evolution of hydrogen that takes place when the mass is treated by water is owing to the potassium, and not to the combustible radical of fluoric acid. It is by the process in question that Davy endeavoured to reduce silica; and he obtained with the mixed silicate of potash only a brown pulverulent matter, which dissolved in the water, giving it a greenish-gray colour. I observed the same colour in the solution; but it disappears when this becomes clear.

I passed silicated fluoric gas through a red-hot iron tube filled with turnings of the same metal, and it was not sensibly absorbed. The turnings when taken out were of a nut-brown colour, similar to that of silicium, and had the taste of fluate of iron, in the places where the heat had had the most intensity. This last having been dissolved by water, a perceptible pellicle of silicium was left on the surface of the iron, but so thin that it was not possible to separate it. We see by this experiment that iron at a sufficiently elevated temperature has an affinity strong enough to decompose the gas; but that the decomposition soon ceases, because the silicium which is deposited at the surface of the metal prevents its action. By fusing in a covered iron vessel a mixture of very fine iron filings with fluate of silica and potash, the salt was decomposed and converted into double proto-fluate of iron and potash; after having dissolved it in warm water, a combination of iron and of silicium was left. I hoped to be able to separate the iron by means of an acid; but the silicium became oxidated at the same time, although I employed the liquid silicated fluoric acid. In drying this alloy it oxidated more quickly in the air than it dried, and was changed into an ochre of a rusty yellow colour.

Composition of Silica.

Since silicium can be obtained in a direct manner, it was natural to endeavor to ascertain its oxidation in a direct manner. For this purpose I burnt 100 parts of pure silicium, dried in a vacuum, by heating it with carbonate of soda. The mass, treated by muriatic acid, evaporated to dryness, and strongly heated, was dissolved in water, and left some silica coloured gray by carbon, which, well washed and ignited, became snow-white, and weighed 203,75. The solution obtained and the waters of the washing were evaporated afresh, and the saline residuum heated to redness. Treating this with water, it still gave a little silica, which, after the addition of a drop of ammonia, took at the expiration of some hours a colour inclining to a yellowish-brown; when heated to redness it weighed 1,5; it had lost its dark colour, but it was not snow-white. With soda it gave on platinum foil a faint but evident trace of manganese. 100 parts of silicium had consequently absorbed 105,25 of oxygen, and produced 205,25 of silica. The experiment was repeated on a portion of silicium on which fluoric acid had been evaporated, in order to be more certain that all the silica had been removed. 100 parts of silicium, previously calcined to redness in the air, gave, by the process described above, 207 of silica; and by evaporation of the waters, 1 part; or in all, 208 of silica.

According to these two experiments, silica is composed of

Silicium . . .	48,72 to 48,08
Oxygen . . .	51,28 to 51,92

They both give a greater proportion of oxygen than that which I found, according to the capacity of saturation of silica for saline bases, and which is only 50,3.

But if we return to the analysis of the salts containing fluate of silica, we shall be able to calculate the saturating capacity of silicium according to the results which we have obtained from it.

Of all these double salts, that of barytes is the most suitable. The only uncertainty which could be met with in its analysis arises from its retaining a little humidity which is not disengaged until the moment of its decomposition. But the quantity of it may be determined by melting the salt with oxide of lead; for the acid is retained, and the water only disengaged. 100 parts of the salt lost in this manner 0,85 of moisture. 100 parts of the same salt, weighed at the same time, gave, according to the process described for the analysis of the fluate of silica and barytes, 82,933 of sulphate of barytes, representing 54,428 of barytes. But according to the analysis already known of the double fluate of silica and barytes, the base is combined with three times more fluoric acid than in the neutral fluate. It thence follows that the double salt is formed of

Barytes	54,428
Fluoric acid	22,836
Silica	21,886
Moisture	0,850
	100,000

The 54,428 parts of barytes are saturated by 7,612 of fluoric acid; whence it follows that 15,224 of this acid were combined with 21,886 of silica, or that 100 of acid took 143,76 of silica. The fluate of silica is consequently composed of

Fluoric acid . . .	410,24	100,00
Silica	589,76	143,76

But 100 parts of fluoric acid corresponds to 74,7194 of oxygen in each base that it saturates; consequently this quantity of oxygen ought to be contained in the 143,76 of silex which the acid saturates, and the composition of silica should be

Silicium	48,025	100,00
Oxygen	51,975	108,22

This proportion comes very near to that obtained by synthesis, but it is difficult to say on which side the greatest errors of observation are to be found. According to what precedes, the weight of the atom of silicium, supposing that the silica contains three atoms of oxygen, would be 277,2, and, according to the best of the two synthetic experiments, 277,8. The other would carry it to 285; but this number by all appearance is too great. It exceeds by $1\frac{2}{3}$ per cent that which had been adopted at first; which is so well suited for the new and exact analyses of minerals, that if they were calculated according to the determination which we have just given, they would render an excess of silica necessary. But I must remark on this head, that a mineral is rarely found of which silica does not form an essential component, which does not yet contain from $\frac{1}{3}$ to 2 per cent of it, and even more, either in the state of quartz or in that of another siliceous mineral. This circumstance must take place so much the more with the minerals which have silica for one of their elements; and consequently all that quantity of it which is found above what the calculation gives should be attributed to a siliceous mixture existing in the mineral.

As to the number of atoms of oxygen contained in silica; the new facts do not furnish us with the means of determining this point. The circumstance that the carburetted silicium produces when burned an equal weight of silica, leads us to consider it as a quadricarburet, which by burning would give an oxide at three atoms of oxygen; but as I have not been able to isolate the carburet of silicium, nor to burn it completely, this result, although obtained in several experiments and with silicium produced by different operations, has not the certitude it ought to have, to be admitted as a proof. It ought notwithstanding to be considered as affording a strong presumption that silica contains three atoms of oxygen, so far as our knowledge respecting the crystalline form of bodies furnishes us with the means of deciding on the number of atoms of which oxides are formed. For the expression of the composition of the silicates by formulæ, it would certainly be more simple to consider silica as formed of an atom of silicium and an atom of oxygen; but it then would be difficult to conceive the existence of the silicates which contain six times the oxygen of the base, as in the apophyllite,—in which an atom of potash would then be combined with 12 atoms of silica.

To conclude: It still remains to be decided to what class of simple bodies silicium belongs. Since it has no lustre, nor the property of conducting electricity in the state in which it has hitherto been obtained, it is evident that it cannot be classed among the metals, and that its properties bring it near to boron and carbon. Some systematical philosophers will in consequence, doubtless, give it the name of *silicon*, to indicate by its termination the class of combustibles to which it should be referred. But I look upon this denomination as useless; for there is not any true limit between the metals and the metalloids. Carbon possesses the metallic lustre, and conducts electricity, but it is not considered as a metal; and if silicium could be melted, it then perhaps would have these properties, which it does not possess in a pulverulent state. Uranium, under this last form, can be distinguished but with difficulty by its appearance from silicium; and when crystallized, on the contrary, it has the metallic lustre and is transparent on the thinnest edges. Columbium and titanium also approach silicium by their chemical properties; and would it not be well to separate them from the metals, in order to unite them to the metalloids,—that is to say, to the non-metallic combustible bodies? I only wish to show, by these remarks, that there is no natural limit between these bodies; and that when their electric relation only is considered as exact, it is quite indifferent whether we place a combustible body among the metals or not.—*Annales des Physik und Chemie.*

Art. XII.—*Observations on the Habits of the Hyæna.* By Robert Knox, M. D. F. R. S. E. Lecturer on Anatomy and Physiology, and Conservator of the Museum of the Royal College of Surgeons. Communicated by the Author. [*Jameson's Edin. Jour.*]

About a year ago, the Wernerian Society of Edinburgh did me the honor to publish in their Transactions, a brief notice by me of the habits of the hyæna, at present existing in Southern Africa, in which I endeavoured to show, 1st, That the Rev. Mr Buckland, in his Geological Speculations, intended not so much to advance science, as to fix the era of a universal deluge, had taken up inaccurate, or at least by far too sweeping views of the natural habits of this ferocious animal; and 2d, That it was of little moment to a geological theory, what were the habits of modern or postdiluvian hyænas, since the antediluvian relics belonged to a different species; which latter circumstance Mr Buckland seems to have forgot, (or perhaps was not acquainted with,) until reminded of it by Dr Fleming. I shall now take the liberty of returning to the subject, and of pointing out to Mr Buckland, (since he has honoured my former brief notice with a short comment,) several facts in the history of these animals, which militate against his "Theory," and render his mode of accounting for the accumulation of bones in the Cave of Kirkdale, absolutely untenable.

1. In the immediate vicinity of the abodes of men, the hyæna usually drags his prey to a considerable distance, *in order that he may not be disturbed at his meal;* but his enormous voracity is a hindrance to his dragging the prey to any great distance, much less to store up food in caves, with a foresight exceeding most of the savage human tribes. Where the country is thinly inhabited, they will eat the carcase of an animal a few hundred yards from the houses; and where there are no houses, they will eat until they are filled to the mouth within thirty yards of a great number of armed men. I saw this happen;—a large striped hyæna was killed, during the night, by a sentinel, within thirty yards of an encampment of nine hundred men; on dissecting the animal, it was very evident that it had been engaged otherwise than dragging the bones to its den, (possibly twenty miles off,) for its stomach was so enormously distended with the offal of the cattle which had been felled all round the camp for the use of the troops, that it seemed, at first sight, to occupy the whole abdominal cavity. Now this dissection was performed by me before hundreds of persons.

2. I feel surprised that Mr Buckland can have forgot the account Bruce gives of a hyæna which ate, during the night, and close to his tent, more than would have sufficed half-a-dozen dogs. It is true, that Mr Bruce's reviewers made it out, that the hyæna had ate more than his own weight; but, setting aside this part of the story, which, of course, is a diverting exaggeration, Mr Bruce might have told his reviewers, that until they had examined the stomach of the hyæna,

they could not well imagine its vast capacity, commensurate with the natural voraciousness of the animal. I could easily multiply these instances from my own experience, for there were killed sometimes four or five hyænas within eight days, close to our habitations. Assuredly these animals were not busied in carrying away bones to remote caverns for future geologists to speculate upon.

3. It is not improbable that, in thickly inhabited countries the habits of the hyæna may be much altered, as we find to be the case in all other wild animals.* When much harassed, they become timed, and fly far from the abodes of men. I should be glad to offer this explanation in support of the supposed habits of the Kirkdale hyænas, but, unfortunately, the antediluvians had not discovered Britain, which, though a fine country, and full of nature's finest productions, did not boast of them as tenants.

4. Hyænas do not *congregate;* they are solitary. Consequently, all that Mr Buckland has said about *a den of hyænas*, is simply the work of the imagination heated by a false theory.

5. The young of the hyæna follow the dam early into the field, so that the quantity of food required to be carried to them must be small.

6. I do not believe that hyænas reside in caverns; they are too timid and distrustful of every thing. The almost inaccessible parts of the country to which they retire during the day-time, must have been visited by very few travellers. I cannot say that I have ever discovered hyænas in dens, though I have often been present when they were roused from their lurking-places.

These are all the observations I intend offering on this subject. It probably did not merit even these, since, as I formerly hinted,† the habits of modern and antediluvian hyænas might be entirely different, as they belong to different species. But however this may be, none who has inspected the Kirkdale bones, deposited in the Hunterian museum, can hesitate for a moment in declaring, that these bones have never been fractured by hyænas; they have been broken by great external violence, and not by the agency of the teeth of living animals; and they do not differ in any respect from the bones found at Oreston and elsewhere, which bear no such marks of violence. It seems indeed strange that the agency of wild animals should be resorted to in explaining the osteological collection of the Kirkdale cave, when so many other collections are allowed to have been formed in an entirely different manner. But the truth is, that we have evidence in the nature of the relics themselves, subversive of Mr Buckland's speculations on these subjects; 1. The bones found in the cave of Kirkdale do not bear the marks of having been broken by hyænas, but of having been dashed to pieces, and exposed to the action of water; 2. They belong to animals of a different species from any which now exist; like the mammoth, they have entirely disappeared from the face of the earth; the very race has been utterly destroyed. But the deluge which effected this, was not the universal deluge described in Scripture, for of all that then lived, and was liable to be destroyed by the devouring element, *individuals were carefully preserved to extend the race of animals to the present day.* Mr Buckland will, I trust, see the propriety of in future avoiding all speculations which have even a remote tendency to mingle up matters esteemed by most men as sacred, with short-sighted human theories, amongst which, I fear, must be ranked that which he has brought forward, and defended with much ingenuity and labour.

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* My friend, Dr Versfield, a native of the Cape, informs me, that he has, on two occasions, observed caves in the Table Mountain, in the immediate vicinity of Cape Town, which contained a few bones, apparently of some ruminating animal; he ascribed the presence of these bones in the caves to the agency of hyænas, with which opinion I entirely agree. When we reflect on the proximity of so populous a town as the Cape to the Table Mountain, it is quite surprising that large collections of bones have not been found in its caves; the number of hyænas inhabiting these mountains must be very great.

† Wernerian Transactions, vol. v.

ART. XIII.—*Abstract of a Series of Papers lately read before the Royal Society, &c. on the Magnetism developed by Rotation.* By Messrs Barlow, Christie, Babbage, Herschell, and Marsh.* [*Philos. Mag.*]

On the Magnetism imparted to Iron Bodies by Rotation. By Peter Barlow, Esq. F.R.S.

THE author's attention having been recalled to the consideration of the effects of rotation in altering the magnetic influence of iron, in the course of speculations on the cause of the rotation of the earth's magnetic poles, and knowing at the same time that Mr Christie had found a permanent change in the magnetic state of an iron plate by a mere change of position on its axis, it seemed to him highly probable that this change, due only to a simple inversion, would be increased by rapid rotation. On trial, however, it was found that the effect produced was merely temporary. The experiments at first were made with a thirteen-inch mortar-shell fixed to the mandrel of a powerful turning lathe, worked by a steam-engine in the royal arsenal at Woolwich.

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* The great importance of the experiments and investigations detailed in these papers, has determined us to give in a connected form the official abstracts of them, as read before the Royal Society, from the Quarterly Journal of Science. We have added, to complete the series, Mr Marsh's accouut of Mr Barlow's repetition of M. Arago's late experiments, from the Edinburgh Philosophical Journal.

This being made to revolve at the rate of 640 turns per minute, the needle was deflected out several degrees, and there remained stationary during the motion of the ball, but returned immediately to its original position on ceasing the rotation. On inverting the motion of the shell, an equal and contrary deflection took place.

As the law of the phenomena was not evident with this disposition of the apparatus, and the shell was found too heavy for perfect safety, a Shrapnell shell of eight inch diameter was mounted in a proper apparatus (described in the paper), and a number of experiments made, the law of which, however, still seemed anomalous, till the idea occurred of neutralizing the earth's action on the needle; when the anomalies disappeared, and the general law of the effect was placed in evidence. The needle being made a tangent to the ball, if the ball was made to revolve *towards* the needle (whatever was the direction of the axis of rotation), the north end of the latter was attracted, and if the contrary way, repelled. In the two extremities of the axis there was found no effect; while in two opposite points at right angles to the axis, the effect was a maximum, and the direction of the needle was to the centre of the ball.

The author then proceeded to show how the results, which before appeared anomalous, agree with this general view, and closed his communication with some theoretical views of their general bearing on the subjects of the earth's magnetism, which he thought there were strong reasons for believing to be of the *induced* kind; and although it appeared to him doubtful whether the anomalies observed in the variation of the needle on the earth's surface can ultimately be referred to this cause, yet he observed that one condition essential to the production of these phenomena holds good in the case of the earth, viz. the non-coincidence of its polarized axis with that of its diurnal rotation.

On the Alteration in the Magnetism of an Iron Plate, occasioned by a Rotation on its Axis. By S. H. Christie, Esq.

The effects observed and described in this paper, although minute in themselves, appeared, in the author's opinion, to point out a species of magnetic action not hitherto described. It has long been well known that striking, twisting, or filing iron, in different directions, with regard to the magnetic axis, materially influences its polarity, but it does not appear to have been remarked that the simple rotation of iron in different directions has any such influence. This, however, the author has ascertained to be the case, and that the laws which govern this peculiar action are so regular, that there can remain no doubt of a corresponding regularity in their causes.

The attention of the author was first drawn to these phenomena by some apparent anomalies in the magnetic action of an iron plate on the compass, observed in the course of a different investigation. In order to avoid or allow for the disturbing influence of partial magnetism in the iron, it became necessary to attend minutely to the position of certain points in its circumference, which corresponded to the maxima and minima of this magnetism. It was then found that these points were not constant, but shifted their position as the plate was made to revolve in its own plane; or, in other words, that a plate which, in a given position, produced a certain deviation in a compass, no longer produced the same deviation after making an exact revolution in its own plane, although brought to rest, and every part of the apparatus restored precisely to its former place.

It appeared from this, that the revolution of the plate in its own plane had an influence on its power of deviating the needle independent of the partial magnetism of particular points in it; and the justice of this idea was proved by giving it a rotation in an opposite direction when the effect on its directive power was also reversed.

The change produced by rotation in the directive power of the plate was found to be a maximum when its plane was parallel to the line of dip on the magnetic axis, and at the same time as little inclined to the horizon as this condition would allow; but when the plane of the plate was parallel to the horizon, the effect was diminished in the ratio of 5 to 1, and when perpendicular to the horizon, and coincident with the magnetic meridian, was altogether destroyed.

The author, having satisfied himself of the reality and constancy of this effect, in different plates, and of the necessity of referring it to a peculiar agency of the earth's magnetic power on the molecules of the plate, proceeded to ascertain the laws, and measure the quantities of the *deviation* due to rotation (so he terms it) in various positions; and detailed a great number of experiments, with their numerical results, arranged in the form of tables.

From these he deduced the following general law; viz. that the deviation due to rotation in *a dipping needle* "will always be such, that the sides of the equator of such dipping needle will deviate in a direction contrary to the directions in which the edge of the plate moves, that edge of the plate nearest to either edge of the equator producing the greatest effect."

The results of this law, it may be here observed, are in many cases coincident with those of the following: conceive the dipping needle orthographically projected on the plate. Then will the *deviation due to rotation* of the projected needle take *place in a direction opposite to that of the rotation itself.*

The author then proceeded to a theoretical investigation of the effect of a plate of soft iron, having within it two poles developed in given positions, and acting (in addition to the usual magnetic action of soft iron) on a needle of infinitely small dimensions, in the plane of the plate. He referred the whole ordinary action of the iron to its centre, and supposed that this is *attractive* on both poles of the needle; but the extraordinary action on that of the newly-developed poles he supposed to reside in them, and to be attractive or repulsive, according as they act on the poles of the needle of the same or opposite names with themselves. On this hypothesis, assuming symbols for the co-ordinates of the plate's centre, the distance separating the newly-developed poles in the plate, and the angle which the line joining them makes with the direction of the needle, &c., he deduced (from the known laws of magnetism) formulæ, expressing the horizontal deviations of the needle:—first, on the supposition of a rotation in one direction; secondly, on that of a rotation in the opposite; and thirdly, in that of no rotation at all. From these, by comparing them with a few of the observations, he deduced numerical values for the constants of the formulæ, and then employed them to compute the deviations due to rotation in all the rest. He regarded the discrepancy between the calculated and observed results, as in few cases, larger than what he considered may be fairly attributed to error of observation; and that the theory above stated is at least a general representation of what passes in fact; admitting, however, that it does not give the exact position of the point where the deviation due to rotation vanishes, and suggesting partial magnetism in the iron plate used as one mode of accounting for the difference. At all events, by an examination of the case on the ordinary supposition of induced magnetism in the iron, he showed that a greater coincidence between theory and fact would not result from that hypothesis than from the one here employed.

He then proceeded to inquire into the degree of permanence of the polarity thus produced in iron by rotation; from which inquiry it appeared that (at least during 12 hours after the plate was brought to rest) the influence of a single rotation had scarcely suffered any diminution. It appeared also that the effect is so far from depending on the rapidity of the motion that the plate can scarcely be made to revolve so slowly as that the whole effect shall not be produced.

Lastly, by a slight change in the formulæ, the results of computation, it is found, can be made to agree with observation to a degree of exactness as near as can be wished. This change consists in the omission of certain terms introduced by the theory, and the author regards it as very possible so to modify the theory as to get rid of them.

The author closed this communication with an appendix comparing the magnetic effects produced by slow and rapid rotation. The result of the comparison was, that the forces exerted on the needle during rapid rotation were always in the same direction as those derived from the slowest rotation, and which continue to act after the rotation has ceased, but were greater in intensity, and that the former effects were such as might have been looked for from a knowledge of the latter.

An Account of the Repetition of M. Arago's Experiments on the Magnetism developed during the Act of Rotation. By Charles Babbage, Esq. F.R.S., and J. F. Herschell, Esq. Sec. R.S.

The experiments of M. Arago having excited much interest, the authors of this communication were induced to erect an apparatus for their verification; and after a few trials, they succeeded in causing a compass to deviate from the magnetic meridian, by setting in rotation under it plates of copper, zinc, lead, &c.

To obtain more visible and regular effects, however, they found it necessary to reverse the experiment, by setting in rotation a powerful horse-shoe magnet, and suspending over it the various metals, and other substances to be examined, which were found to follow with various degrees of readiness the motion of the magnet. The substances in which they succeeded in developing signs of magnetism were, copper, zinc, silver, tin, lead, antimony, mercury, gold, bismuth, and carbon in that peculiar metalloidal state in which it is precipitated from carburetted hydrogen in gas works. In the case of mercury, the rigorous absence of iron was secured. In other bodies, such as sulphuric acid, rosin, glass, and other non-conductors, or imperfect conductors of electricity, no positive evidence of magnetism was obtained.

The comparative intensities of action of these bodies were next numerically determined by two different methods, viz., by observing the deviation of the compass over revolving plates of great size cast to one pattern, and by the times of rotation of a neutralized system of magnets suspended over them; and it is curious that the two methods, though they assigned the same order to the remaining bodies, uniformly gave opposite results in the cases of zinc and copper, placing them constantly above or below each other according to the mode of observation employed.

Our authors next investigated the effect of solution of continuity on the various metals; in the course of which M. Arago's results of the diminution of effect by division of the metallic plates used were verified; and the further curious fact ascertained, that re-establishing the metallic contact with other metals restores the force, either wholly or in great measure; and that even when the metal used for soldering has, in itself, but a very feeble magnetic power, thus affording a power of magnifying weak degrees of magnetism. The law of diminution of the force by increase of distance was next investigated. It appears to follow no constant progression according to a fixed power of the distance, but to vary between the square and the cube.

The remainder of this paper was devoted to some able and elaborate reasoning on the facts detailed.—The authors conceive that they may be all explained without any new hypothesis in magnetism, by supposing simply that time is requisite both for the developement and loss of magnetism; and that different metals differ in respect, not only of the time they require, but in the intensity of the force ultimately producible in them; and they apply this explanation not only to their own results, but to those obtained by Mr Barlow in his paper on the rotation of iron.

Experiments on the Magnetism produced by Rotation. By S. H. Christie, Esq., in a Letter to Mr Herschel.

Mr Christie, in this communication, gave an account of some experiments on the developement of magnetism in copper by rotation. He corroborated by his own experience the results obtained by Mr Herschel, in which a disc of copper was set in rotation by the rotation of one or more magnets beneath it, both in the case where poles of the same name were immediately below the disc, and when of a contrary name. The actions appeared equally intense in both cases; and from this circumstance, he concludes the magnetism thus communicated to the copper to be extremely transient. The experiment was varied by combining the revolving magnets differently, and the results were stated.

The next experiments of Mr Christie were directed to the determination of the law according to which the force diminishes as the distance between the disc and magnets increases. It seems to follow from these experiments, that when a thick copper plate is made to revolve under a small magnet, the force tending to deviate the needle is directly as the velocity, and inversely as the fourth power of the distance; but that when magnets of considerable size are made to revolve under thin copper discs, the diminution follows more nearly the ratio of the inverse square of the distance, or between the square and the cube, though not in any constant ratio of an exact power.

The author then investigated the law of force when copper discs of different weights are set in rotation, which, for small distances, appear proportioned to the weights of the discs, but for smaller ones appear to vary in some higher ratio.

Account of the Repetition of M. Arago's Experiments on the Magnetism developed during the Act of Rotation. By Messrs Barlow and Marsh.

The experiments by Mr Barlow on the magnetism imparted by rotation, described in p. 124, were begun in Dec. 1824; and it was not until April 1825 that he was informed of M. Arago's rotative experiments on copper and other metals. "The latter were not known in England," Mr Marsh states, "until M. Gay-Lussac's visit to London at the time above stated."—"I am not aware," he continues, "of the precise nature of these experiments; and shall, therefore, only endeavour to describe those which I have assisted Mr Barlow in making, and which he founded on the description he had received; they may, therefore, be considered as the experiments of M. Arago repeated, and varied as different circumstances occurred to suggest new ideas. The account he had of M. Arago's experiment, was that, by placing a copper plate upon a vertical spindle, the plate being horizontal, and then placing just above it a light compass needle, but independent, of course, of the plate; on causing the spindle and plate to revolve, the needle was considerably deflected, and more and more as the velocity was increased; so that, when the plate was put into rapid rotation, the needle also began, after a few vibrations, to revolve, and at length with considerable velocity.

"1. In order to repeat this experiment, I connected the wheel of my turning lathe with a vertical spindle, which I could make revolve forty-five times per second; and on this I placed a thin copper plate, about six inches in diameter, and over this a needle about five inches long, shut up in a close box, about one inch, or rather less, above the plate. When putting the lathe in motion, I found it to deflect the needle about five points, the deflection being always in the same direction as the motion of the plate, but we could not cause it to revolve. The needle was, therefore, partly neutralized by a bar magnet, and the experiment repeated. We then very soon obtained a considerable rotatory motion in the needle; and, by using a larger and heavier plate, the same was produced afterwards without neutralizing the needle.

"2. Another experiment, which was mentioned as one of M. Arago's, and which I repeated, was, by interposing a plate of iron between the copper plate and the needle. In this case, no effect could be produced on the needle by the rotation of the copper plate, the iron clearly intercepting the action.

"3. The only other experiment that I am aware of as originating with M. Arago, at least that I repeated, was the rotation of a plate cut into radii like a star, which was said, as I understood, to produce no effect; this, however, was not the case in my experiments,—it certainly produced a less effect, but, I think, not less than might have been anticipated, from the quantity of copper thus taken away.

"4. I now tried a zinc plate instead of a copper plate, and the effect was nearly the same as before, but a little less.

"5. An iron plate was now substituted, and the effect was considerably greater than with the copper plate.

"6. The copper plate was again replaced, and a brass needle placed in the box. Some motion was obtained, but it was very equivocal, so that I cannot venture to say that it was certainly due to the rotation.

"7. A heavy horse-shoe magnet was now suspended by a line from the ceiling; and it was put in rotation by the revolution of the copper plate, a paper screen having been first interposed between them.

"8. One copper plate was suspended over another, but no motion was obtained; and the same took place when the copper plate was suspended over an iron one.

"9. A bar magnet, rather shorter than the diameter of the copper plate, was fixed horizontally to the upright spindle; and being made to revolve, the plate very soon acquired rotation. A paper screen was, in this, as in the preceding experiments, interposed between the plate and magnet.

"10. The plate was now applied immediately to the axis of the lathe, so as to cause it to revolve vertically, and the needle placed near to it; but no motion took place, till, by nearly neutralizing the needle, and bringing either of its poles directly to the plate, it then always deviated in the direction of the motion of the plate; whichever pole of the needle was directed to the former. The needle, of course, therefore, deviated different ways (all other things being the same), when it was above or below the axis; but in the direct horizontal line of the axis no motion in the needle took place.

"11. The above are the principal experiments that I assisted in making by revolving the plate; but these having suggested to Mr Barlow that all the results obtained might be explained, by supposing that there existed a slight magnetic power in copper, and in the various metals which had a tendency to draw the needle after the plate, or the latter after the former, he endeavoured to exhibit this by direct experiment, independent of revolution. With this view, he neutralized a needle very accurately; and then applying very near to its poles the end of a round brass ruler, the attraction of the latter was obvious,—it drew the needle several degrees, —then, withdrawing it, and catching the needle again in its returning vibration, it was drawn out some further degrees; and, in a very short time, the deflection was converted into a revolution, which, by alternately presenting and withdrawing the needle, was at length rendered very rapid.

"12. The same result was obtained by two or three different pieces of brass; but there were other pieces, although of the same size and form, which had little or no effect.

"The following experiment is due to Mr Sturgeon, of Woolwich.

"13. A thin copper plate or wheel, about five or six inches in diameter, was suspended very delicately on an axis, and then one side a little weighted, in order to give it a tendency to oscillate. The heavy point was now raised level with the axis, and the number of vibrations the plate made before it came to rest were counted. The same was again done, with this difference only, that the vibrations now took place between the poles of a horse-shoe magnet; and the number of them before the plate came to rest, was very little more than one half of what they were in the former instance.

"This is the converse of M. Arago's experiments, in which he shows the effect of copper and other metallic rings, in diminishing the number of oscillations of a magnetic needle.

"14. If, instead of a horseshoe magnet, the contrary poles of two bar magnets be used, the effect is the same as before; but, if the poles of the same name, viz. both north or both south, be employed, then the effect is scarcely perceptible. This is an important result, as it shows that the effect is not due to any kind of resisting medium, as was supposed in the first instance."

Art. XIV.—*Researches on a New Acid universally diffused through Vegetables.* By M. H. Biaconnet. (*Philos. Jour.*)

I first obtained this acid from the tubercles of the dahlia and artichoke, whilst engaged in their analysis, but ignorant of most of its properties, I neglected to insert it as a constituent principle. Some time after, whilst examining the roots of cultivated celery, I found the same principle, and observed its acid properties. It has, since then, occurred so frequently, that I have not met with a single vegetable or succulent root without observing its presence.

I have found it in the roots of the turnip, carrot, phytalacca, (phytalacca decandria,) scorzonere hispanica?) piony, phlomede (tubereuse) in the roots of patience (Rumex patienza,) and of filipen dule (saxifraga rubra) where it is united to a colouring principle, in bulbs, the onion; in the stalks and leaves of herbaceous plants; in the cortical layers of all trees previously deprived of the coloured external bark, from whence it may be obtained in great abundance, sometimes united to a red colouring matter, as in the cherry-tree, maple, nut-tree, at other times colourless nearly, as in the elder tree; in sawdust of the maple; in apples, pears, prunes, cucurbitaceous fruits, and, without doubt, in all other fruits, and in grain.

Before describing the properties of this acid, I should state, that it appears to me very analogous to, if not identical with, the principle so little known under the vague term of jelly. However that may be, it is easily obtained from all parts of plants. If roots containing starch be operated upon, such as those of celery and carrot, they are to be reduced to pulp by rasping, the juice expressed, the residue boiled with water, slightly acidified with muriatic acid, then washed, and afterwards heated with a very dilute solution of potash or soda; a thick mucilaginous liquid results, slightly alkaline, from which muriatic acid separates the new acid in the form of an abundant jelly, which should then be well washed. In this state it is almost colourless, especially when from colourless vegetable substances; it has a sensibly acid taste, and applied to litmus paper, distinctly reddens it, though containing no foreign acid.

It is scarcely soluble in cold water, but a solution may be obtained, sensible to re-agents. Boiling water has a stronger action; the filtered liquor is colourless, deposits nothing on cooling, and barely reddens litmus; alcohol coagulates it into a transparent jelly, colourless as ice; so also do all metallic solutions, lime-water, baryta-water, acids, muriate and sulphate of soda, nitrate of potash, &c. This acid appears to be held so feebly in aqueous solution, that the solution of a little sugar is sufficient to make the greatest part of the liquor coagulate. Dried in a capsule, it appears in transparent films having no adhesion to the vessel. When dry, it scarcely swells in cold water; dissolves in small quantities in boiling water, and offers the phenomena just described. The acid precipitated from its potash combination by muriatic acid, when distilled did not swell up, and gave a product containing much empyrumatic oil, but no ammonia nor muriatic acid; much charcoal remained. Diffused through water, and aided by warmth, it disengages carbonic acid from its alkaline combinations.

It forms a very soluble salt with potash, which may be obtained in the state of transparent jelly, by adding weak alcohol, which removes the excess of alkali, and colouring matter, if there be any. This jelly, washed on a cloth with alcoholized water, pressed, and dried, is a neutral combination, which swells and dissolves in water, and leaves upon evaporation of the liquid a transparent mass, full of cracks, and resembling gum arabic in appearance; it has so little disposition to adhere, that the slightest friction removes it entirely from the capsule. The taste of this salt is insipid; put upon a plate of red-hot iron, it swells excessively, leaving a deep-brown residue, soluble in water, and having the character of ulmin, united to potash. Exposed to the flame of a taper, it burns, producing delicate filaments, which project out from the mass like vermicelli. These filaments fuse into globules of sub-carbonate of potash.

This salt, in aqueous solution, is gelatinized by alcohol, sugar, muriate of soda, acetate of potash, and other neutral salts. All earthy and metallic salts decompose it by double affinity. The acids unite to the alkali, and separate the acid as a jelly. One hundred parts of the neutral combination burnt in a platina crucible, left a quantity of sub-carbonate of potash, which, heated to redness with sulphuric acid, gave 28 parts of sulphate of potash, from which it would appear, that the salt was formed of

Acid	85
Potash	15
	100

Let it not be supposed that this salt will be useless; I am persuaded that it will have many applications in the art of the confectioner. It is, indeed, remarkable, that so small a quantity of the combination can communicate the property of gelatinizing to such large quantities of sugared water. I dissolved one part of this salt obtained from the turnip root in a certain quantity of warm water; sugar was then dissolved in the liquor, and finally a very small quantity of acid added; an instant afterwards, the whole became a trembling gelatinous mass, weighing 300 parts. I have in this prepared aromatic jellies, perfectly transparent and colourless, and very agreeable to the taste and sight. By colouring rose-water with a little cochineal, I have also prepared jelly of roses, of an exquisite taste.

The same acid added to a weak solution of ammonia, forms also a soluble compound, which evaporated to dryness, leaves a neutral insipid salt, something like gum, which separates from the evaporating dish in large plates, like mica. Its solution coagulates with different substances in the same manner as the preceding. I dissolved one part of this combination in 100 parts of water, and added alcohol, which separated a transparent jelly; this, drained on a cloth, weighed 110 parts. This salt will answer like the preceding in the preparation of jellies; and indeed is more advantageous since it may be readily obtained in the neutral state.

The other combinations of this acid are almost all insoluble and may be obtained by double decomposition.

Concentrated sulphuric acid appears to have but little action when cold on this acid; by means of heat it produces sulphurous acid and ulmin. Nitric acid distilled to dryness with it leaves a residue, which with water yielded oxalic acid and a white powder, which was partly dissolved by ammonia, and left oxalate of lime. An acid added to the ammoniacal solution separated a granular sediment, crystalline acid, and having the character of mucous acid; heated in a glass tube, it blackened, fused, swelled up, and produced acicular crystalline sublimate.

Such are the principle properties which I have observed in this acid. It may be presumed that a principle thus universally spread over all vegetables, has an important part to play, and merits the serious attention of physiologists. I am very much disposed to think that it is the same thing as the *cambium* or organizable substance of Gren and Duhamel, and which appears, as is known, in gelatinous drops. Wherever new developements are forming, I shall endeavour in the spring to ascertain whether my conjecture is well-founded. In the mean time, I propose the name of *pectic acid*, from *πηκτις* (*pēctis*) coagulum, to distinguish it from other bodies of the same class.

ART. XV.—*On some Kinds of Fulminating Powder inflammable by Percussion, and their Use in Fire-arms.* By P. W. Schmidt, Lieutenant in the Prussian service.* (*Philos. Mag.*)

A POWDER inflammable by percussion, has been used for some years past, especially in fowling-pieces. The following formulæ have been given for the preparation of this powder, the principal ingredient of which is chlorate of potash.

1. 100 parts of chlorate of potash (fulminating salt,) 12 parts of sulphur, and 10 parts of charcoal are closely mixed. The grains are produced by forcing the soft paste through a sieve.

2. 100 parts of chlorate of potash, 42 parts of saltpetre, 36 parts of sulphur, and 14 parts of lycopodium.

These are the usual ingredients that have hitherto been mixed with the chlorate of potash for the purpose of making priming powder. The guns, however, with which this powder is used, are very various in their construction. In some it explodes of itself by means of the mechanism of the lock, on being cocked, into a small conical recess, which communicates with the touch-hole, in others, it is put in previous to every shot. In the former kind of guns a quantity of powder sufficient for a certain number of shots is kept in a recess attached to the lock, called a magazine; and the locks (which were invented in England by Mr Forsyth) are called maginine locks.

In some guns the stroke of the cock, which is in the shape of a hammer, falls immediately on the fulminating powder strewed in the above recess. In order to protect the powder from wet, small balls of it were covered over with wax, and placed sometimes in the conical recess, and at others fixed to the cock itself. In both instances the ball was kindled in the recess just mentioned, by means of the percussion.

Besides these, other contrivances have been used for the purpose of igniting this kind of powder; yet they have all their defects, and offer so many difficulties in practice as to have prevented their general introduction.

Latterly, they have contrived in Germany to fix the powder in a small case of very thin copper foil, for the purpose of keeping it dry; and for that purpose a cylinder is screwed into the body of the gun instead of the touch-hole, and rests for the sake of greater support on the plate of the lock, instead of resting on the pan. The inner space of the cylinder is filled in loading with the same powder as that of the shot. The igniting box, at the bottom of which is the detonating powder, is, previous to firing, upturned on the cylinder. In this cylinder is a small round aperture leading to the inner space of the cylinder. On the trigger being pulled, the cock strikes the igniting-box, and the fulminating powder is kindled by the blow, flows through the aperture, inflames the shot, and breaks the igniting-box.

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* From Schweigger's Journal, Band xi. p. 66.

Mr Wright seems to have taken great pains with the subject.* He recommends, for the firing cases, to use fulminating mercury, saying that sportsmen had justly complained of the powder made of chlorate of potash, since it soon produces the oxidation of the barrel and touch-hole, and the charcoal which remains after the firing, rendering them unfit for use. The advantages of his new powder he enumerates as follows: It does not make the gun rusty so soon as the other; it produces neither dirt nor moisture; it is not liable to explode as the other powder, and if it does explode its effects are less destructive, inasmuch as its power does not extend so far.

The following is his mode of preparation:

"I place two drachms of quicksilver in a Florence flask, and pour six drachms (measure) of *pure* nitric acid on the mercury; this I place in a stand over a spirit-lamp, and make it boil till the quicksilver is taken up by the acid; when nearly cool, I pour it on an ounce (measure) of alcohol in another flask: sometimes immediate effervescence ensues, with the extrication of nitrous æther; and often I have been obliged to place the mixture over the lamp, till a white fume begins to rise, when the effervescence follows. I suffer the process to continue (removing the lamp) till the fumes assume a reddish hue; when I pour water into the flask, and the powder is found precipitated to the bottom, I pour off and add fresh water, permitting the powder to subside each time before the water is poured off, so as to free the substance as much as possible from the acid, and then I pour it on a piece of filtering paper, and place the powder in an airy room to dry. It should be kept in a corked (not stoppered) bottle."

For the filling of the cups, he makes use of an ivory pin, which has a scoop at one end for the purpose of receiving the powder, and at the other is cut flat. With it he puts in as much fulminating mercury as will cover the bottom; he then dips the flat end into a strong tincture of benzoin, and rubs this substance gently about the case; by which means the powder is set fast and covered as with a varnish.

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* Mr Wright's paper will be found in Phil. Mag. vol. lxii, p. 203.

Professor Schweigger, speaking of these kinds of experiments in his chemical lectures, observed on the practicability of kindling gunpowder by the admixture of other substances, as has been shown in a criminal investigation that took place at Munich a few years ago. A box filled with gunpowder was sent to an individual provided with fulminating papers, which were to inflame on the box being opened. Fortunately, however, the murderous design was frustrated; for although the papers exploded, they did not kindle the powder. The assassin was discovered and punished. M. Gehlen, who had been examined at the trial, was led by the circumstance to make several experiments for the purpose of kindling gunpowder by means of Brugnatelli's fulminating silver, but they all failed.

It seems that in England, too, difficulties had been found in igniting gunpowder with fulminating mercury; for Mr Wright observes, "if any one doubts the practicability of firing gunpowder by means of fulminating mercury, by procuring a percussion-gun, he may try the experiment and be fully satisfied."

Professor Schweigger having, therefore, requested me to try some experiments on this subject, especially with fulminating silver, I made them in the chemical laboratory of our university, and the following were the results.

I. Fulminating silver was prepared in the usual way; five drachms of fuming nitric acid, and five of alcohol were poured over one drachm of fused nitrate of silver. When the effervescence and solution were complete, water was added. The precipitate of fulminating silver was filtered off, and all the remaining acidity washed from it with water. The liquid which had passed through the filter gave with muriatic acid a copious precipitate of chloride of silver. The fulminating silver, which was of a whitish tint, was now subjected to the following experiments:

1. When damp it ignited very rarely, and only by a strong blow. When dry, it explodes easily, and with a much slighter blow.

2. When touched with sulphuric acid, it exploded equally strongly, whether damp or dry.

3. Damp or dry, it exploded in the fire.

4. The substance which remained after the ignition, was of a bluish brilliant hue and a disagreeable metallic taste. I could gather but little, which, dissolved in water, produced a faint red tint on litmus paper.

5. I failed in several attempts to ignite gun-powder with the fulminating silver. I therefore put quantities of the size of a small pin's head, into some copper boxes, fastening it in some with tincture of benzoin, in others with a solution of gum arabic in water; and others I tried to press on the bottom without any other aid. I applied them to guns prepared for the use of igniting-boxes, and thus kindled the gunpowder with incredible swiftness. The series of experiments thus made in the presence of Professor Schweigger, leave no doubt that fulminating silver will easily ignite gun-powder in a mannner that will secure it against a rapid dispersion on exploding.

II. The fulminating mercury was prepared in the manner prescribed by Mr Wright. But I must observe that the experiment only succeeded by the application of fuming nitric acid. The fulminating mercury, when obtained, was washed till every particle of acidity had disappeared from it. It was then submitted to the following experiments:

1. When dry, it exploded like fulminating silver, and with a much slighter blow than required for the powder made of chlorate of potash. Thus it would appear as if the fulminating quicksilver had not in this respect the advantage over the igniting powder made with the salt just named.

2. When perfectly dry only, it could be ignited with sulphuric acid.

3. In the fire it exploded, both wet and dry.

4. The substance remaining after the explosion, had a bluish brilliant tint and a bitter acid metallic taste. With a small quantity dissolved in water, litmus paper was slightly reddened. I made no further investigation, inasmuch as the examination of the acids of fulminating metals, before and after the explosion, was not my object; especially since Dr Liebig has lately published a series of very interesting experiments, the repetition of which would require very extensive labour.* Liebig calls those acids fulminic acids, which

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* Vide *Ann. de Chim. et de Phys.* part xxiv. p. 294, or the translation in Gilbert's *Annalen der Phys.* part lxxv. p. 393—422. Mr Liebig found that Brugnatelli's detonating silver was dissolved in lime-water or solutions of the caustic alkalies, whereby

being the property of all metals, he distinguishes into silver and mercury fulminic acids, &c.

The great advantages, however, of the fulminating quicksilver as igniting powder, extolled by Mr Wright, I did not find confirmed; although I proceeded to fill copper boxes, as I had done with the detonating silver, which all ignited the charge.

III. I also submitted to experiment the first-named mixture, principally consisting of chlorate of potash; and found

1. That it exploded only by a hard blow. Its effects were much less than those of the detonating silver or quicksilver. Mr Wright, indeed, says the contrary of the latter; it seems, therefore, that I used a better kind of fulminating mercury; but for that very reason I must give the chlorate of potash the preference for practical use.

2. In the fire it puffed away like gunpowder.

3. The substance remaining after the explosion is blackish and dusty, and contains less of acidity than that left by the quicksilver. Thus, and indeed from all my experiments, it is evident that it oxidizes the iron less than the fulminating mercury. Moisture is also left by the latter; and the charcoal left by the mixture of chlorate of potash, after kindling a copper box filled with it, is very unimportant:—therefore this mixture is preferable as an igniting powder. This is also the reason why the manufacturers no longer use the fulminating quicksilver. I know one who makes and fills weekly several thousands of copper caps, for which he uses the chlorate

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31,25 per cent of oxide of silver was deposited. They produce peculiar salts, called *fulminates*, which explode with great violence. These salts are dissolved by nitric acid, sulphuric acid, and acetic acid; the silver fulminic acid contained in them, and so difficult to be dissolved, is deposited; and by heating a solution of fulminate of lime to the boiling point, and adding a moderate quantity of nitric. acid is deposited, on cooling, at the bottom of the vessel, in the shape of long white crystals. This acid may be easily dissolved in boiling water; from which it crystallizes again in cooling, has a disgusting metallic taste, and reddens litmus paper. But it cannot subsist of itself without combination with a metal; and in the same manner as there are prussic acids of iron, copper, silver, and gold, so the fulminic acid combines with silver, quicksilver, copper, iron, zinc, &c., into proper fulminic acid, which again form different combinations with the bases, *e. g.* potash, soda, barytes, strontian, lime, &c. Thus, for instance, silver-fulminate of potash consists of 35,03 parts of silver-fulminic acid and 14,92 of potash; silver-fulminate of soda, of 88,66 parts of silver-fulminic acid and 11,34 of alkali. When cooling, Berthollet's detonating silver forms granular shining white crystals. One part of this salt makes as violent a report as three parts of Howard's (Brugnatelli's) fulminating silver With magnesia the silver-fulminic acid combines in two ways. One combination is a simple decrepitating, not detonating, insoluble powder, of a rosy tint; the other forms white capillary crystals, and explodes very loudly. The first combination was used for the analysis of fulmic acid in the dry way; in which the fulminating silver was evinced to consist of 32,22 of oxygen, 3,22 of hydrogen, 11,28 of azote, 9,68 of carbon, and 41 of silver.

mixture, the preparation of which is both less expensive and less dangerous than that of the fulminating quicksilver. There is another circumstance attending this mixture. In filling the caps, it will sometimes happen that the quantity put in is doubled, which I find is of no injurious consequence with this mixture; but might endanger the person firing with the fulminating mercury, as the box will burst too violently.

With respect to the power of igniting the charge, the different kinds of powder which I have compared are equally effectual.

IV. I submitted the mixture of chlorate of potash mentioned above to the following experiments.

1. That part only ignited which was struck, without igniting that lying around it.

2. In the fire it burns away with noise.

3. I placed it in the usual way in copper caps, but could not ignite a charge with them. The cause of this may be explained by the construction of the locks, with reference to the properties of this detonating powder. That part of the cap situated just above the opening of the cylinder remains, as the blow cannot fall on it unignited, as shown by the experiment No. 1. But the communication of the ignited part with the charge, is prevented by the manner in which the cock strikes the cap. In guns in which such powder is used for igniting, it lies as above stated, in small balls in a conical aperture. Here it is nearly all ignited by the striking of the cock, and must of necessity flow inwards, every other way of escape being shut up.

In conclusion:—I have to add that the method of filling the caps recommended by Mr Wright is not only laborious, but even dangerous. How are manufacturers to employ that method when they have to fill several thousands a week? I have made various trials, and the following process seems to me to be the best.

Pour some adhesive solution or tincture over the powder, and mix it into a stiff kind of liquid. Take with a brush or a stick a large drop of it, and apply it against the bottom of the cap.

This method is both quick and free from danger; whilst on filling with the dry detonating powder, the least careless touch may produce an explosion.

In order to prevent the corrosion of the cylinder, and its becoming useless by the formation of sulphuret of iron (an evil very common with iron touch-holes, and caused more by the action of the gun-powder than by that of the igniting substance), the inside of the cylinder should be lined with a metal which will neither oxidate nor easily combine with the ingredients of the powder.

Art. XVI.—*On the Analysis of Atmospheric Air by Hydrogen.* By John Dalton, Esq. F. R. S. &c. (*Ann. Philos.*)

According to my promise, I transmit the results of some late experiments on the analysis of atmospheric air by hydrogen. My chief object was to find under what circumstances the union of the oxygen and hydrogen, by the electric spark, is complete, that is, so that one or both of the gases are entirely consumed; and in what cases either no union takes place or a partial one, leaving portions of both gases still in mixture in the residue.

From a memoir of MM. Humboldt and Gay-Lussac (*Ann. de Chimie*, 53, 1805,) we learn that one volume of hydrogen, mixed with two or nine volumes of oxygen, gives the same loss by electricity, namely, 1,46; but if mixed with 9,5 oxygen, the loss is only ,68; and this loss diminishes rapidly, till the oxygen becomes 16, when there is no loss at all. They found that if the surplus gas was azote or carbonic acid, the loss was not much different; but they do not seem to have ascertained this with precision.

It is right to observe that the hydrogen I used was obtained in the usual way from zinc and diluted sulphuric acid, and was received in bottles filled with as pure rain-water as I could procure; the bottles were filled with the gas, and not more than one third of the gas of each bottle was used; the hydrogen was free from atmospheric air, except what was expelled from the water by the hydrogen bubbling into the bottle: this quantity of atmospheric air, however, must be something; yet on firing 10 measures of hydrogen with oxygen, the diminution is usually 14,6 to 15.

The mixtures of gases fired at once were commonly about 150 measures, each measure being the volume of one grain of water. The eudiometer has six inches in length correspondent to 150 measures, and all the experiments were made over water.

The atmospheric air I mostly used was procured in the country, and was found by frequent trials to contain almost exactly 21 per cent. of oxygen. This is not the case at all times. I once found the oxygen as high as 21,15 per cent. from an average of many experiments; it was on the 8th of January last, when the barometer was 30,9, wind N.E. and very moderate, after three days of calm and gentle frost. But the general state of the atmosphere yields only 20,7 or 20,8 per cent. of oxygen. All the results below must be considered as averages of four or five experiments.

Tabular Arrangement of the Experiments.

Measures of Oxygen.		Measures of Mixtures		Azote.	Hydr.		Measures.	
1	to	14,0	=	(3,76 +	10,24)	Does not explode		
1		13,0	=	(3,76 +	9,24)	Fires imperfect	Loss 2,7....0 &	h
1		12,0	=	(3,76 +	8,24)	" perfect	" 3,0.. .0	h
1		11,0	=	(3,76 +	7,24)	"	" 3,0....0	h
1		10,0	=	(3,76 +	6,24)	"	" 3,0....0	h
1		9,0	=	(3,76 +	5,24)	"	" 3,0....0	h
1		8,0	=	(3,76 +	4,24)	"	" 3,0....0	h
1		7,0	=	(3,76 +	3,24)	"	" 3,0....0	h
1		6,0	=	(3,76 +	2,24)	"	" 3,0....0	h
1		5,86	=	(3,76 +	2,10)	"	" 3,0....0	n
1		5,76	=	(3,76 +	2,00)	"	" 3,0....0	*o*
Hydr.					Oxg.			
or 1		2,38	=	(1,88 +	0,5)	"	" 1½....	*o*
1		3,00	=	(2,37 +	0,63)	"	" 1½....	*o*
1		4,00	=	(3,16 +	0,84)	"	" 1½....	*o*
1		5,00	=	(3,95 +	1,05)	"	" 1½....	*o*
1		6,00	=	(4,74 +	1,26)	" rather imper.	" 1½....	*o*
1		7,00	=	(5,53 +	1,47)	" imperfect	" 1,0....0	h
1		8,00	=	(6,32 +	1,68)	"	" 1,0....0	h
1		9,00	=	(7,11 +	1,89)	"	" 0,75....0	h
1		10.00	=	(7,90 +	2,10)	"	" 0,60....0	h
1		11,00	=	(8,69 +	2,31)	"	" 0,45....0	h
1		12,00	=	(9.48 +	2,52)	"	" 0,30....0	h
1		13,00	=	(10,27 +	2,73)	"	" 0,20....0	h
1		14,00	=	(11,06 +	2,94)	Does not fire sometimes; fires at others.	" 0,20....0	h

N. B. Those experiments marked *h* denote that hydrogen remained in the residue after detonation; those marked *o* denote oxygen; and that marked *n* denotes that neither of the two was found.

direction of that point in the heavens, to which the magnetic needle is directed, when suspended by its centre of gravity, in such a manner as to admit of its taking its position freely, in obedience to the resultant of the magnetic forces exerted upon it by the terrestrial globe. I have myself* had an opportunity of verifying most of the particulars here mentioned in the case of a very large aurora borealis, which was visible on the 27th of August, 1817, during my visit to the Shetland Islands.

We first saw in the northeastern parts of the horizon, several slender jets of light, which, having attained a little elevation, continued to shine for some time and then vanished; but in about an hour and a half afterwards they re-appeared in the same region of the heavens, and were now much stronger, more brilliant, and more extended. Very soon a regular arc resembling a rainbow began to present itself just above the horizon. It was at first incomplete, but gradually increased; and after some moments, I saw the other part approaching from the west, and upon being formed, it ascended instantaneously, accompanied by a multitude of jets of light which rushed towards it from all parts of the northern horizon; then the summit of the curvature rose almost to the zenith. This arc was at first wavering and unsettled, as if its component parts had not taken a stable position; but very soon the agitation entirely ceased, and it remained in undisturbed beauty for more than an hour, having only a progressive motion, and that almost insensible, towards the southeast, whither it seemed to be carried by a gentle north-western breeze that was then blowing. So that I had sufficient time to examine it, and to fix its limits and position with the circle used in my astronomical observations. I found that it comprehended a portion of the horizon amounting to 123° 42′, and that its centre was situated exactly in the direction of the magnetic needle. The whole region of the atmosphere embraced by this arc in the north-western part of the heavens, was incessantly traversed in all directions by luminous jets, whose different forms, motions, colours, and durations, engrossed my imagination no less than my senses. Most frequently, each jet at its first appearance was a mere stream of whitish light; its size and brightness rapidly increased, and it occasionally presented some very singular variations of direction and curvature. When completely developed, it contracted into a slender rectilineal thread, for the most part exceedingly brilliant, and tinged with a very deep red colour. After this it grew fainter and fainter till it finally vanished, often at the very place where it first appeared. The long continuance of many jets in the same apparent place, considered in connection with the infinity of shades assumed by them, seems to prove, that the light is not reflected, but direct, and that it is actually developed in the place where it is first seen; besides, I have not been able to discover in it the least trace of those physical properties which characterize reflected light; and which are designated by the term *polarization*. All these fires and even the arch that comprehends them, occupy a region more elevated than the clouds; since the clouds themselves intercept them; and the edges of these clouds were actually or seemed to be tinged with light. The moon, which had then reached a considerable elevation above the horizon, shed her lustre also on this imposing scene, and the tranquility of her silver light formed a most agreeable contrast with those vivid corruscations with which the atmosphere was inundated.

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* T. B. Biot.

Having now given a view of the principal circumstances attending this phenomenon, we propose to deduce from them the conditions of its existence; and the first thing to be determined is, whether it exists in our atmosphere or beyond it. There is a simple method of settling this question. If it be beyond the atmosphere, it must be independent of the diurnal rotation of the earth; and therefore its jets of fire, its arcs, its luminous crowns, must follow the general course of the stars from east to west, and seem like them to turn about the celestial poles. On the contrary, if the meteor belongs to our atmosphere it should partake of the common motion which the rotation of our globe communicates to all terrestrial bodies, even to the clouds; it should then appear to be immovable with respect to these bodies, or at least to undergo only accidental disturbances like the clouds themselves. All observations unite in establishing the latter supposition; and the length of time during which the meteor, observed by me at the Shetland Islands, continued, would, if necessary, afford a fresh confirmation.

We may then consider it as an established fact, that the phenomenon of the aurora borealis takes place in our atmosphere. But, as is well known, elevated objects when seen at a distance through the atmosphere, are apt to produce many optical illusions. For example, all the stars seem to us attached to the concave part of the same spherical surface or dome; although their distances are infinitely various. The vast trains of luminous vapour which form the tails of the comets, seem also to apply themselves to this dome, although in reality they stretch into space in rectilineal directions. By another illusion, when the sun is partially concealed behind a mass of clouds, and emits rays of light through the openings of these clouds, the rays, although actually parallel, appear to converge towards the point of the heavens where the sun is. These general laws of perspective must affect, in like manner, the appearance of the luminous jets emitted by the meteor in question, and must be taken into consideration in our attempts to explain them. But from whatever situation these jets are observed, they always seem to describe arcs of great circles on the celestial dome, and to converge towards that part of the heavens to which the needle points when perfectly free. Whence we conclude, that they are in reality cylindrical, and parallel to the direction of the needle. But each jet presents, moreover, great varieties of size and lustre, from which we are led to believe that they are, in fact, composed of a great number of shorter cylinders independent of each other, and in part piled one above another. As these indications are noticed throughout the whole region of space where the meteor is visible, we may conclude, with geometrical rigour, that it consists of a forest of luminous columns, all parallel to the resultant of the magnetic forces, and of course for short distances parallel to each other, and suspended at nearly equal heights on different sides of the horizon. These columns being situated at different distances from the observer, must, by the perspective effect, appear to be raised to different heights. They must also mutually cover each other, and appear to project one over the other, especially when, being seen near the horizon, the visual rays proceeding from them are nearly perpendicular to their length; but after attaining such an elevation that their intermediate spaces may be seen, they must appear to separate; if then a certain number of them be simultaneously transported over the head of the observer, in such a manner as to pass by the point of the heavens to which the magnetic needle, parallel to them directs itself, the projection of all these columns on the celestial dome, will form about this point a luminous crown, the divergent rays of which will seem to descend on all sides toward the horizon, till they arrive at the apparent height at which the meteoric columns will have descended by the effect of the progressive motion.

This constitution of the meteor, which has been deduced from optical considerations, is rendered probable by many curious facts, which different observers have had occasion to notice, and which have a relation to the positions which these different parts of the meteor happened accidentally to have with respect to them.

For example, when the meteoric colonnade, already illuminated, is situated entirely in the horizon exactly north of the observer, if it happens to be transported in a southerly direction, and in consequence to approach the observer, without any disappearance, or change of arrangement, of the columns composing it, we ought to expect the same optical effect which is presented by the trees of a forest when we approach them; that is, the columns situated eastward will separate toward the east, and the columns situated westward of the plane of the magnetic meridian, will appear to separate toward the west, while those which are in this meridian will appear to be stationary, or at least only to ascend directly towards the zenith. This appearance was attentively observed by F. C. Mayer, at Petersburg, in a large aurora borealis, which was seen on the 16th of Sept. 1726. I will quote his very language, observing that by the word "trabs," he designates a vertical jet, or one of our luminous columns. He first describes the formation of an arc, whose summit was not directed exactly to the north, but which had a very considerable declination to the west. He then adds, "Motus trabium mirus erat; quæ enim in occidentali arcûs parte extabant, versùs occidentum ferebantur; ad orientem ferebantur, quæ in orientali arcûs parte sitæ erant; boreales autem trabes stabant immobiles. Ex hoc phænomeno intellexi lucem moveri ex nord-west versùs verticem meum, id quod sequentibus phænomenis confirmatum est." It will be seen that Mayer has deduced precisely the consequences which are required by the rules of perspective.

Another case which may sometimes present itself, although very rarely, occurs when the illumination of the meteoric colonnade, seemingly accidental, appears for some time to take place only over a certain number of the columns which compose it. Then if these columns are placed at sufficient distances from each other, we may have an opportunity of examining them singly. This opportunity was afforded by the remarkable aurora borealis of 1716, an account of which may be found in the memoir of Dr Halley, (Phil. Trans. 347, p. 411, 415.) Small columns of equal lenghts and parallel to each other were distinctly seen separate in a portion of the heavens surrounded by two luminious and almost horizontal belts. An account of a like phenomenon may also be found in another memoir of Dr Halley, (Phil. Trans. No. 363, p. 1099, for the year 1719.) He there relates, that from time to time, there appeared in the air at a great height collections of columns, or co-ordinate luminous beams, resembling the pipes of an organ, which presented themselves to view as suddenly as if a curtain had been drawn from before them. Indeed, if any one will undertake to read the numerous accounts of this meteor which have been furnished by those who have visited the northern regions, he will find a mass of facts which perfectly answer to the constitution of the meteor as deduced by us from the laws of perspective, and he will not meet with any thing opposed to our conclusions. A full statement of these geometrical deductions has been given by Dalton, probably without being aware that they had been obtained already by Cotes, in 1716, the person of whom Newton said, that "if he had lived, we should have known something;" and that they had since been adopted by Cavendish, the most severe and cautious of all philosophers. I have made this remark in order to show that they may be regarded as rigorous.

After having given a general description of the meteor, one of the most essential circumstances to be determined is its elevation. Attempts have been made without number to ascertain this point, by the aid of the same processes which geometry affords for measuring the distances of inaccessible objects; that is, by observing in different places, at the same time, the position of the same part of the meteor. But the difficulty of obtaining this perfect identity as to time and point of the object, renders the application of the method very uncertain; and accordingly the results obtained by it assign to the meteor uncertain heights, varying, in some cases, from twenty to more than one hundred leagues. Still more uncertainty prevails with respect to the length of the meteoric columns, which some have attempted to measure by like processes. If, in fine, the estimates made under certain favorable circumstances appear worthy of confidence, it may be urged, I think, that they are not general; and that in certain cases at least, the meteor descends much lower than we should thus be led to suppose. This seems probable from the quick and continual agitation of the phosphoric jets, the simultaneous progressive motions of the arcs, like that which a gentle breeze might be expected to give them, the slow and regular transfer of those fleecy portions of phosphoric matter, which travellers in the northern regions assure us they have often seen floating separately in the atmosphere; and I myself saw a like phenomenon at the Shetland Islands, the 6th of September, 1817. It was a dense cloud which slowly ascended above the horizon from the north-east. Its sides were the centres of a phosphoric light which seemed at one time to remain behind till it was extinguished, at another to break forth and illuminate the edges of the cloud. I can give no better idea of this phosphorescent appearance, than by comparing it to the dark clouds of our theatres when illumined by lamps from behind. Yet for some moments I observed on its inferior surface a small spot where the light seemed to intervene between it and me. This cloud, having attained a height of about 45°, remained for some time stationary, and then gently moved to the west, still retaining its phosphorescence; some jets of light also, proceeding from the northern horizon, inclined towards the west, as if a wind in the higher regions of the atmosphere, coming from the south-east, was transporting the meteor to other countries. Similar phenomena presented themselves on the 14th of September. These observations, from which we may infer, that the aurora borealis belongs to the region of the higher clouds, seem to me to render probable an opinion generally prevalent in all northern countries, which is, that the aurora borealis, when very vivid, is accompanied with a considerable noise, and in some cases with one of great violence. I am well aware how little reliance is to be placed on common opinion under circumstances calculated to inspire terror, or when influenced by the frightful appearance of rapid and unexpected commotions; but the assertions thus made, like all others, possess a degree of credibility; and if it is unphilosophical to believe without proof, it is equally so to reject without examination. Let a person apply himself for thirty years to the study of what are called popular prejudices, and I doubt not his labours would be rewarded by many valuable discoveries. If any one will inquire without bias or prepossession into the reality of the sounds alleged to proceed from the aurora borealis, I am persuaded that he will not hesitate to adopt the common opinion, so striking is the coincidence of testimony on this subject. The distinguished natural philosopher Muschenbroek, who wrote about the middle of the last century, reports, that this fact is generally affirmed by sailors employed in the whale fishery on the coast of Greenland. Gmelin, in his account of Siberia, expresses himself in still more decided language; after speaking of the great splendor of the aurora borealis, as presented in these countries, he adds; "However beautiful this spectacle may be, I think it will be impossible to contemplate it for the first time, without emotions of terror; so constantly is it accompanied, as I have been informed by several intelligent persons, with noises like those hissings and cracklings produced by very large fireworks. The hunters who go in search of the blue fox to the confines of the Frozen Ocean, are frequently surprised by the unexpected appearance of this meteor; their dogs are frightened by it to such a degree that they cannot be kept from stopping and lying on the earth until the noise has ceased." There is a phrase belonging to the language of this country, used solely to express the terror which this phenomenon occasions. Gmelin adds, that there was a unanimous voice in support of what is here stated. I can affirm, moreover, that among the inhabitants of the Shetland Islands, the testimony is no less full and complete, although they do not speak of so loud a noise; a difference to be attributed undoubtedly to the less northern situation of these islands. M. Edmonston, who, like myself, was unacquainted with the passage just quoted from the work of Gmelin, described to me the noise occasioned by the aurora borealis in very similar terms, giving me to understand that he had very frequently heard it himself; he thought it most like the noise proceeding from a large fire. I did not have an opportunity of observing it during the appearance of the meteor when I was at Unst, as the sea then roared with great violence on the side of the island where I was. In fine, the inhabitants spoke only of having heard the noise of the meteor, when the phosphoric jets are very numerous, and when they cross and intermingle with the greatest activity. For the truth of what is here alleged, we may appeal with confidence to the whole population of the Shetland Islands; hardly a person is to be found who will deny having heard this noise; we do not however depend on assertion merely; it is described in the same manner by different persons, without their once imagining that there can be any doubt about it. The phenomenon seems to be much more brilliant a few degrees nearer the pole. M. Edmonston, in an account of the appearance of a large aurora borealis which he observed at Unst on the 1st of November, 1818, has afforded me a striking example of this difference. "I am now in company," says he, "with two credible persons, who on a voyage from London to the Shetland Islands, were driven by winds to the latitude of 63½°, near the northernmost point of the islands. While they were in this latitude an aurora borealis appeared; the noise with which it was accompanied was such that the sailors were afraid to remain on deck; and it sent forth so strong a light, that we were able to observe the compass by it." It seems probable after this mass of testimony, that the meteor sometimes descends so low as to allow us to hear the noise proceeding from it. It has even been affirmed by Bergmann, that persons travelling over the Norwegian Alps have been enveloped in it, and have perceived a strong smell of sulphur, supposed to come from it.

Having thus collected the several particulars belonging to the aurora borealis, in doing which, I have endeavoured to exclude every thing of a hypothetical nature, we may consider this meteor as consisting of real clouds, proceeding usually from the north, and composed of some very light substances or at least of some substance so finely pulverized as to be capable of floating a long time in the atmosphere, endued with the property of occasionally becoming luminous; and especially (which is very important) sensible to terrestrial magnetism, and spontaneously arranging themselves in columns which turn towards the earth, as real magnetic needles would do. But of all terrestrial substances only the metals, so far as we know, are in any considerable degree susceptible of magnetism. It is then probable, that the columns of the meteor are at least in a great measure composed of metallic particles reduced to powder of extreme fineness. But this conclusion leads also to another; we know that all known metals are excellent conductors of electricity. Now the different strata of which the atmosphere is composed are usually charged with very unequal quantities of electricity; for if, when the atmosphere is most serene, we raise a paper kite with a metallic string, we may observe at the end of the string signs of electricity, ordinarily of the vitreous kind; and if, on the other hand, having ascended in a balloon, we let fall below the car a wire whose inferior extremity shall reach the lower strata of the atmosphere, we shall find, as has been observed by M. Gay-Lussac and myself, that the superior end of the wire gives indications of resinous electricity. Accordingly, if columns consisting in part of metallic substances, are suspended in nearly a vertical position in the atmosphere, like the columns of the aurora borealis when they float over regions adjacent to the pole, the electricity of the atmospheric strata at the summit and base of the columns will find in them so many conductors more or less perfect; and if this tendency of electricity to diffuse itself uniformly is sufficient to overcome the resistance arising from the imperfect conducting power of the columns, it will flow along these columns, illuminating its path, as is often observed in conductors which are not continuous. When this passage takes place in the higher regions of the atmosphere where the air, on account of its rarity, offers very little resistance, the electricity will flow on silently with all those variations of light which we observe in exhausted tubes. But if it extends itself to the inferior strata, it must necessarily occasion such hissing and crackling noises, as are found to accompany the aurora borealis, when it descends near the surface of the earth. In fine, as the meteor is visible only by means of this accidental circumstance, there is reason to believe that it may exist in the air and exert an influence over the magnetic needle without being perceived; it is also very possible that it may be bright in some places and obscure in others; while under certain circumstances the disturbance of the electric equilibrium being sudden and general, the whole meteoric colonnade may be instantaneously illuminated. These phenomena must be less striking as the meteor advances over the more southern countries, not only because it has then extended itself more widely, but especially because the conducting columns, always conforming to the direction of the magnetic needle, will become more and more horizontal, and will have their two extremities in atmospheric strata less distant, and therefore less unequal with respect to the quantities of electricity with which they are charged; a greater humidity also which prevails in the lower latitudes is favourable to a frequent discharge.

All these results, agreeing so exactly with what we have collected from actual observation, it will be seen, depend solely on the idea, that the columns of which the aurora borealis is constituted, are partly, at least of a metallic nature. This agreement with known phenomena considerably increases the probability of the supposition to which we were previously led by the magnetism of the meteoric columns; the mutual connection and intimate dependance, thus easily established between phenomena so numerous, and, at first view, so remote, give an air of reality to the whole, seldom to be met with in physical theories which have not the basis of established fact.

But, independently of the luminous jets which may thus be produced by the simple passage of electricity along the metallic columns, a passage which in virtue of a property lately discovered, might of itself be sufficient to magnetize these columns; we can hardly help considering the phenomena in question as proceeding from an actual combustion in the phosphoric clouds, which, detaching themselves in some cases from the burning meteor, as affirmed by many observers, and as I have myself seen, transport with them the principle of their phosphorescence, and emit at intervals jets of light resembling rockets, which leave after them a whitish train. We must then regard it as at least a very probable supposition, that the aurora borealis is composed of substances, capable, occasionally of inflammation, either of a spontaneous kind, or in consequence of a discharge of electricity from the clouds which contain it; a very powerful mode of combination, of which we have frequent instances in our laboratories.

Such are the physical conditions on which the aurora borealis seems to depend, and which are deduced directly from the phenomena presented to us. Whence then is derived the matter which constitutes it? To this question we can as yet give only a doubtful answer; but if a skilful observer would, for several winters, carefully study every circumstance belonging to these phenomena as they present themselves in the northern regions, with all the helps that the sciences can furnish, something decisive would probably be learned.

Art. XV.—*On the Mirage.* Communicated for the Journal,

[A few years ago, as the inhabitants of a small village on the coast, not many miles east of the Piscataqua river, were issuing from the village church on Sunday, after services, on a fine summer-morning, they were attracted and alarmed by the vision of a ship, with her sails set and filled, moving magnificently in the air above the ocean, and directing her course westward, towards the land. For a few moments they continued in silent wonder; but a light breeze from the land sprang up, the glorious object vanished in the soft air, and no trace was left of its appearance, but the superstitious awe which remained in the minds of the spectators. This is not a solitary instance. Similar appearances have been at other times observed or related, but have been usually ascribed by the sceptical part of the world, to some foolish imagination or some idle fear that possessed and deceived the minds of the vulgar.

Again, stories have been told of men with a power of vision so penetrating, that they could descry a vessel, and predict her approach when she was many leagues at sea, beyond the reach of the eyes and glasses of all around them. Witchcraft or previous intelligence or conjecture have been brought in to account for the fact, or it has been disbelieved and rejected.

It is easy to say that we do not believe; it is more philosophical often, to confess that we do not understand, and to suspend our judgement until we have examined all the circumstances; and we may sometimes find that researches in the kingdom of nature bring to light principles and facts which give a probable foundation for the common belief of the multitude; as the progress of discovery in modern times has shown to be true, what were long received only as the old man's stories of the garrulous father of history. This we shall find to be true with regard to the exaggerated accounts of the appearance spoken of above. Stripped of its popular dress, we shall find it to take its place among a thousand similar facts perfectly well established. Of the most remarkable of these facts and appearances, it is proposed to give some account. We are led to do this at the present time, not from having any new facts or views to communicate, but from having had our attention drawn to it by lately observing the phenomenon, and finding from conversation that very few of those otherwise conversant with science seemed well to understand the cause or to have read the best accounts that have been given of it. It is known among sailors by the English word *looming*, or by the French word *mirage*, under which name it has been spoken of by men of science.]

The first well described observations on the mirage, or extraordinary refraction, were made by Mr Joseph Huddart, at Allonby, in Cumberland, in 1793.* He observed, at different times, a point of land and a vessel, seen at the distance of about seven leagues over the water. Inverted images of objects were seen immediately under the real objects. He supposes these images to be produced by rays passing downwards from the objects, and being refracted upward again, in a path convex to the earth, by passing through a medium of air, of which, in consequence of evaporation, the greatest density was not at the surface, but a few feet above the surface of the water. This medium he thinks would give an upward curvature to the rays, by which means this would come to the eye as from some point below the real object, and as if reflected from a surface a few feet above the true surface of the water.

In this way he accounts for the blunt appearance of the really sharp points of a promontory seen at a distance; the object seen by the eye being made up of a true point and its reflected image. He supposes, with apparent reason, that this principle will sometimes cause lights at a distance to seem brighter than when seen nearer, as the real and reflected or refracted image, will, in that condition, become nearly coincident.

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* See Philosophical Transactions for 1797, p. 29.

In the summer of 1798, the French army under Bonaparte passed through the desert of Lower Egypt, where they daily observed the phenomenon of which the following description is given by Monge.*

"The country of Lower Egypt is nearly a level plain, which loses itself, like the sea, in the clouds at the extremity of the horizon; its uniformity is only interrupted by a few eminences, either natural or factitious, on which are situated the villages, thus kept out of the reach of the inundations of the Nile.

"Both morning and evening the aspect of the country is exactly as it ought to be; and between you and the last villages which present themselves, you perceive nothing but land; but when the surface of the earth is sufficiently heated by the rays of the sun, and indeed until it begins to get cold towards the evening, the land no longer seems to have the same extension, but to be terminated to within the distance of about a league, by a general inundation.

"The villages placed beyond that, appear like so many islands stationed in the midst of a great lake, from which the spectator is separated by an extent of land, more or less considerable according to circumstances. You then behold the image of each of these villages reflected exactly as if it were exhibited on a clear surface of water, with only this difference, that as the representation is at a considerable distance, the smaller objects are invisible, and the masses alone distinct; in addition to this, the edges of the reverted image are ill defined, and such as they would be if the surface of the reflecting medium happened to be a little agitated.

"In proportion as one approaches a village, which appears to be placed in the midst of an inundation, the margin of the water seems to recede, and the arm of the sea, separating you as it were from the village, shrinks back by degrees; it at length disappears entirely, and the phenomenon which now ceases, in respect to the first village, is instantly reproduced by a new one, which you discover at a due distance in the rear. Thus every thing contributes to complete an illusion, which is sometimes cruel, more especially in the desert, because it tantalizes you with the appearance of water, at a time when you experience the greatest want of that element."

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* Memoirs relative to Egypt, p. 74.

This is explained by Monge by supposing that the air in contact with the earth is heated to a very great degree by the direct rays of the sun, and so continually heated, that, although a portion of it is constantly rising, in consequence of its greater levity, still the portion nearest the earth is, taken altogether, a more rare medium than that at a greater distance. Now when light passes from one medium into another, or from a medium of one degree of density into a medium of another, a portion of the light is reflected at the common surface of the two media. The quantity of light reflected is in proportion to the magnitude of the angle of incidence. The light coming from the lower regions of the heavens strikes this surface of lighter air at a great angle, and consequently the image of the lower part of the sky is reflected to the eye of the observer. But this reflection can take place only at a considerable distance from the eye of the observer, as it is only at such a distance that the angle of reflection or its equal, the angle of incidence, is sufficiently large. All the rays which reach the eye from any point on the surface nearer than a league's distance, come under an angle less than the lowest limit of reflection.

If the same phenomenon should take place in a situation where there were no elevated objects to show the true limits of the horizon, the sky would seem to be prolonged, the apparent horizon would be depressed, and would seem much nearer than it really is. But where villages or trees occur beyond the nearest limit of reflection, they point out the real situation of the horizon, and the image of the heavens which is seen within that horizon, will at once assume the appearance of a surface of water. Terrestrial objects occurring in such a situation will seem to be reflected from the image of the sky between the observer and the objects, in the same manner as they would be from a surface of water, with this difference, that the reflected image will be less distinct, and will have the appearance of vibrating, from the undulating motion of the surface of air from which they are reflected.

The next observations are the extremely curious but inaccurate ones, of which the following account is abridged from a very interesting paper in the Philosophical Transactions for 1799, p. 13. In this paper, Prof. Vince details some observations made at Ramsgate, in a sultry evening, after an extremely hot day, in August, 1798, under a clear sky, from elevations sometimes of 25, at other times of 80 feet above the water, with a telescope magnifying 30 or 40 times.

He first directed the telescope towards a ship, the topmasts of which were just visible above the horizon. At the same time he discovered in the field of view, two complete images of the ship in the air, directly over the ship itself, the lower image being inverted, and the upper one erect, having their hulks joined. As the ship was receding from the shore, less and less of its masts became visible, and, as the ship descended, the images ascended, but as it did not sink entirely below the horizon, he had not an opportunity of observing in what order and at what time the images would in that case have vanished.

On directing his telescope towards another ship, whose hulk was visible in the horizon, he observed a complete inverted image, the mainmast of which just touched that of the ship itself, and which moved as the ship moved, without any variation in its appearance. In this case there was no second image.

The next ship to which he directed his telescope was so far on the other side of the horizon, as just to prevent its hulk from being seen. At first, only an image of the topsail and of the mast, joining that of the ship, and an image of the bowsprit were visible. These images would suddenly appear and disappear very quickly after each other, first appearing below and "running up very rapidly, showing more or less of the masts at different times, as they broke out; resembling, in the quickness of their breaking out, the shooting out of a beam of the aurora borealis." The ship was receding, and he found that, as it descended beyond the horizon, more of the image gradually appeared; till an image of the whole ship was completed, with the mainmasts touching each other. As the ship descended lower, the image and ship separated, but no second image was observed.

On moving the telescope along the horizon, he observed the top of the mast of a ship, just visible. Directly over this, at some little distance, was an inverted image of a vessel, and above that, an erect image, both of them very perfect and well defined, and an image of the sea between them, the water appearing very distinctly. As the ship approached the horizon, the upper, erect image gradually disappeared, and vanished, and after that the image of the water disappeared, and the lower, inverted image descended. He thinks that if he had directed his telescope to that point sooner, he should have seen the two images before any part of the ship itself was visible.

Upon turning the telescope towards the cliffs at Calais, which were visible, he observed an image of the cliffs above the cliffs themselves, with an image of the sea between them. Afterwards an inverted image of a ship appeared against the cliffs, while the hulk of the ship was below the horizon. He supposes from the appearance of the fog soon after in that direction, that there was, at the time he made these observations, a considerable fog on the other side of the horizon.

These observations were often repeated, and the images in general well defined. The usual refraction at the same time was so great, that the cliffs at Calais were visible at the water's edge, whereas they are frequently not to be seen in clear weather, from the high lands about the place.

To *explain* these appearances, Prof. Vince supposes three rays to reach the eye from the object. One ray is subject to the ordinary refraction, and exhibits an image in the natural way, a little above the true place of the object. A second ray is supposed to pass above the first ray through air "varying quicker in its refractive power" than the air through which the first ray passed, and therefore having a greater curvature, the convexity being upwards and presenting a second image; "in like manner, if a quicker variation of refractive power should take place above this curve," a third ray may describe a third curve still higher, also convex upwards, and presenting a third image still higher than either of the former. "The alterations of refractive power," Prof. Vince supposes, "may arise partly from the variations of its density and partly from the variations of its moisture, and the passages of the rays through the boundary of the fog, may there suffer a very considerable refraction."

In Scoresby's voyage to Greenland, in the summer of 1822, he observed many curious phenomena of this kind, which he has extremely well described. The first description we shall extract is the following:

"In the evening it was calm, with a brilliant sun, and very curious exhibitions of the phenomena of unequal refraction. Hummocks of ice assumed the form of castles, obelisks, and spires; and the land presented extraordinary features. In some places, the distant ice was so extremely irregular, and appeared so full of pinnacles, that it resembled a forest of naked trees; in others it had the character of an extensive city, crowded with churches, castles, and public edifices. The land was equally under the influence of this singular mirage. Huge masses of rocks and summits of mountains were reared to an enormous elevation, distorted into singular shapes, and often seemed to be detached from the rest of the land, and freely suspended in the air. The horizon, bounded by ice, which ought to have been pretty regular and uniform, was sometimes undulated and broken."

Another scene observed within a few days of the same time, may be found described in page 217, Vol. I, of this Journal.

He describes the following appearances as having been observed on July 9th.

"The morning was brilliant, with light winds, and a highly refractive state of the atmosphere. The land, and a number of ships that came within sight, as well as the ice, and indeed all distant objects, were strangely distorted. Inverted images of two ships, occasionally double, were seen in the air, which, I imagine, were at least ten miles beyond the limit of direct vision; for we approached them about this distance without being able to see them. In addition to the phenomena observed and described on the 19th of June &c. I noticed several vessels that had their hulls elevated to the apparent magnitude of a castle; the height of the hull, in some instances, being equal to that of the masts; in two or three positions, the courses seemed to be separated twenty or thirty yards from the hull, instead of being nearly in contact. So unequal was the refraction, and so various in its effects, that while in one ship the masts were uniformly expanded, or the hull magnified,—in another ship the courses and topgallant-sails were heightened, and the intermediate sails, the top-sails, contracted to one fourth their proper size,—and in a third, a very little distance from this, the courses and topgallant-sails were contracted, and the top-sails expanded. In all these examples, the peculiarities were continually varying. No sooner had one appearance been examined and sketched, than it changed, and often exhibited the most uncouth proportions. The distant ice partook also of the same influence, and presented very extraordinary and often beautiful architectural structures. These interesting exhibitions were at length closed by the commencement of a dense fog." p. 143.

Again,

"As soon as the fog had cleared away, on the 16th, the influence of the sun that was then most powerful, soon began to produce a rapid evaporation from the surface of the sea and ice,—this gave rise to the optical phenomena of unequal refraction, which continued to operate with ever varying effect, for sixty or seventy hours. At one period (about 10 P. M. of the 16th) the phenomenon was so universal, that the space in which the ship navigated seemed to be one vast circular area, bounded by a mural precipice, of great elevation, of basaltic ice. In some places, where water, instead of ice, was seen through the refracting medium, the cliff it seemed to constitute had the colour and appearance of real basaltic columns; and where both water and ice were refracted, the specks and patches of ice had sometimes so much of the character and appearance of land, that one of my principal officers, who was familiar with the general phenomena, was deceived by it."

On the 18th, he says,

"This evening, about 9 P. M., the refracted ships, with their repeated images, were most beautifully and distinctly defined, and especially the latter, which appeared as usual, in an inverted position in the air. Of some vessels, whose hulls were beyond the horizon, there were two, and of one ship three, distinct inverted images, each exhibited in a different stratum of refracted ice, one above another,—the lowest image being at an altitude of more than the apparent height of the ship's mast, above the mast-head of the original. And of two vessels there were well defined images, in an inverted position, though the ships to which they referred were not within sight! It should be observed, that the inverted images were visible on this occasion, only, when an appearance of ice, produced either by reflection or refraction, occurred above the regular line of the horizon, in the quarter occupied by the ships: in the clear intervals of the lower atmosphere between the strata of refracted ice no image was seen; and when the stratum was too narrow to comprise the whole of the image, a part of it only appeared. And it should be also observed, that these phenomena were principally telescopic, both the ships and the images being so distant, that, to the naked eye, they only appeared as indistinct specks."

The appearance of the land was no less remarkable than that of objects at sea.

"In many places there were patches resembling two obtuse pyramids united by their apices, the upper one, though quite as distinct as the other, being evidently the inverted image only of the lower one. In occasional positions, where two of these double pyramids were near together, the upper limbs of the higher pyramids coalesced, so as to present the appearance of prodigious bridges, some leagues in extent, with a clear atmosphere beneath them. Sometimes these pyramids were so compressed and multiplied, that three or four were seen in a vertical series, forming so many distinct horizontal strata, joining in the middle, but all detached at the extremities.

The general telescopic appearance of these coasts, when under the influence of unequal refraction, is frequently that of an extensive ancient city, abounding with the ruins of castles, obelisks, churches, and monuments, with other large and conspicuous buildings. Some of the hills often appear to be surmounted with turrets, battlements, spires, and pinnacles; while others, subjected to another kind of refraction, exhibit large masses of rock, apparently suspended in the air, at a considerable elevation above the actual termination of the mountains to which they refer. The whole exhibition is frequently a grand and interesting phantasmagoria. Scarcely is the appearance of any object fully examined and determined, before it changes into something else. It is, perhaps, alternately a castle, a cathedral, or an obelisk; then expanding and coalescing with the adjoining mountains, it unites the intermediate valleys, though they may be miles in width, by a bridge of a single arch of the most magnificent appearance."

"The cause of these phenomena, as far as they depend on refraction, is," Scoreby supposes, "the rapid evaporation which takes place in a hot sun from the surface of the sea; and the unequal density occasioned by partial condensations, when the moist air becomes chilled by passing over considerable surfaces of ice." This is sometimes visible to the eye and gives a tremulous motion to all bodies on or near the water. It does not, however, he supposes, extend to a great distance above the surface, since objects as elevated as the top of the mast, from which most of his observations were made, were often not affected by it, while every object near the surface was very much so. The medium of the atmosphere, near the water, he supposes to be made up of parallel strata of different densities, often very unequal and changeable. Objects seen through such a medium must be unequally elevated and distorted, while objects seen through a medium of a uniform density, or of a density uniformly increasing or diminishing, would be regularly elevated to a less or greater degree, in proportion to the density of the medium and the distance of the object.

In the following paragraph, Scoresby gives the reason why this curious phenomenon has been so seldom noticed or described by navigators.

" As the more extraordinary and beautiful effects of unequal refraction cannot be fully discovered, without the use of a telescope, they escape general observation; and as the looming of the distant ice, or distortion of objects, is the most common effect of this state of the atmosphere, it becomes a considerable annoyance to persons not interested in the phenomena. As in this case, the vapour gives an indefinite and tremulous outline to every object beyond a certain distance; —ships, a mile or two off, cannot be recognised;—a wall of ice seems to surround the navigator, the openings and leads in which cannot be discerned at a distance;—and, of the actions and employment of remote vessels, within sight, a knowledge of which is often of great importance to the unoccupied fisher, no correct conception can be formed.

Some of the appearances described by Scoresby have a strong resemblance to the phenomenon which is seen at times in the Straits of Messina, and which has been long known and often described under the name of the Fata Morgana.

One of the earliest descriptions of it, is that of father Angelucci, as quoted in Swinburne's Travels.

"On the 15th of August, 1643, as I stood at my window, I was surprised with a most wonderful, delectable vision. The sea that washes the Sicilian shore swelled up, and became, for ten miles in length, like a chain of dark mountains; while the waters near our Calabrian coast grew quite smooth, and appeared as one clear polished mirror, reclining against the aforesaid ridge. On this glass was depicted, in *chiaro scuro*, a string of several thousands of pilasters, all equal in altitude, distance, and degree of light and shade. In a moment they lost half their height, and bent into arcades, like Roman aqueducts. A long cornice was then formed on the top, and above it rose castles innumerable, all perfectly alike. These soon split into towers, which were shortly after lost in colonnades, then windows, and at last ended in pines, cypresses, and other trees, even and similar This was the Fata Morgana, which, for twenty-six years I had thought a mere fable."*

A very full and philosophical account is given of it by Minasi.

" When the rising sun shines from that point whence its incident ray forms an angle of about forty-five degrees on the sea of Reggio, and the bright surface of the water in the bay is not disturbed either by the wind or the current, the spectator being placed on an eminence of the city, with his back to the sun and his face to the sea;—on a sudden there appear in the water, as in a catroptric theatre, various multiplied objects; that is to say, numberless series of pilastres, arches, castles well delineated, regular columns, lofty towers, superb palaces, with balconies and windows, extended alleys of trees, delightful plains with herds and flocks, armies of men on foot and horseback, and many other strange images, in their natural colours and proper actions, passing rapidly in succession along the surface of the sea, during the whole of the short period of time while the abovementioned causes remain.

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* Polehampton's Gallery, vol. p. 513.

"But if, in addition to the circumstances before described, the atmosphere be highly impregnated with vapour and dense exhalations, not previously dispersed by the action of the wind or waves, or rarefied by the sun, it then happens that in this vapour, as in a curtain extended along the channel to the height of about thirty palms, and nearly down to the sea, the observer will behold the scene of the same objects not only reflected from the surface of the sea, but likewise in the air, though not so distinct or well-defined as the former objects from the sea.

"Lastly, if the air be slightly hazy and opake, and at the same time dewy and adapted to form the iris, then the abovementioned objects will appear only at the surface of the sea, as in the first case, but all vividly coloured or fringed with red, green, blue, and other prismatic colours."*

Minasi supposes these different appearances to be produced by the reflection of objects on the neighbouring coasts from the surface of the water in the strait, by refraction produced by saline and other effluvia, that is, by evaporation and by fogs.

(To be Continued.)

ART. XIV.—*Notice of the Periodical Comet of* 1819.† By M. Damoiseau. (Translated for this Journal from the *Connaissance des Tems*, for 1827. By Seth Ames, A. B.)

SOON after the discovery of this comet by M. Pons, at Marseilles, it was ascertained that its movement was periodical, it had already been observed in 1786, 1795, and 1805, and was afterwards observed at 1822, at Paramatta. In order to be able to predict its next return to its perihelion, it is indispensably necessary to ascertain the irregularities produced in its motion by the disturbing force of the planets. The observations of 1786 were not sufficient to ascertain its orbit, and those of 1795, exhibit but little precision; but the elliptical orbits of M. Encke, which represent so exactly the observations taken in 1805, 1819, and 1822, have served as the basis of the actual research. I present, in a number of tables, the results of a calculation, relating to the alterations of the comet's orbit in these two last periods, and the two following, with their application to its future state in 1825 and 1828. The method and the details of these different calculations may be found in a memoir presented to the Academy of Sciences.

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* Polehampton's Gallery, p. 511.

† This notice is an extract from a memoir presented by M. Damoiseau to the Academy of Sciences, in the month of February last, and is the subject of M. Poisson's report, which may be found, p. 224 *Connais. des Tems.*

Jupiter, the Earth, and Venus are the only planets whose action has been considered; the following are the results in relation to the mean anomaly and mean dialy motion.

Periods.	Intervals observed.	Alteration of the Mean Anomaly.		Alteration of mean daily Motion.
1805 to 1819	4814′,746	By the action of ♃	+15859″38	+ 3″5889
		♀ in 1809	— 476,76	— 0,1313
		in 1818	— 4,97	— 0,1105
		♁ in 1806	+ 64,01	+ 0,0178
		in 1818	— 5,14	— 0,0735
			+ 15436,52	+ 3,2914
1819 to 1822	1212′,742	By the action of ♃	— 9939″38	— 7″4349
		♀ in 1819	+ 81,26	+ 0,0716
			— 9858,12	— 7,3633
1822 to 1825	- - - - -	By the action of ♃	+ 363″51	+ 0″7202
		♁ in 1822	— 56,90	— 0,0494
			+ 306,61	+ 0,6708
1825 to 1829	- - - -	By the action of ♃	— 705″25	— 04″420
		♁ in 1828	— 6,90	— 0,0986
			— 712,15	— 0,5406

If we designate the mean daily motions of the comet at its perihelion that of

1805 by n
1819 n'
1822 n''
1825 n'''
1829 n''''

and the intervals between passing the perihelion, from

1805 to 1819 by T
1819 1822 T'
1822 1825 T''
1825 1829 T'''

we shall have by the period from 1805 to 1819.

$$n = \frac{560^\circ \times 4 - 15436,''52}{T} = 1076'',6925 - 3'',2061$$

$$= 1073'',4864,$$

$$n' = 1073'',4864 + 3'',2914 = 1076'',7778,$$

$$n'' = 1073'',4864 + 3'',2914 - 7'',3633 = 1069'',4145;$$

by the period from 1819 to 1822.

$$n' = \frac{360^\circ + 9858'',12}{T'} = 1068'',6525 + 8'',1288$$

$$= 1076'',7813,$$

$$n'' = 1076'',7813 - 7''3633 = 1069'',4180,$$

$$n = 1076'',7813 - 3''2914 = 1073'',4899;$$

uniting the two periods, we have for the alteration of the mean anomaly in the interval between the perihelia of 1805 and 1822.

$$15436'',52 + 3'',2914 \times T' - 9858'',12 = 9570'',02$$

which gives

$$n = \frac{360^\circ \times 5 - 9570'',02}{T + T'} = 1075'',0745 - 1'',5877$$

$$= 1073'',4868,$$

$$n'' = 1073'',4868 + 3'',2914 - 7'',3633 = 1069'',4149,$$

$$n' = 1073'',4868 + 3'',2914 = 1076'',7782.$$

the values which are found have been formed into a table.

Periods.	n	n'	n''
1805 to 1819	1073'',4864	1076'',7778	1069'',4145
1819 to 1822	1073'',4899	1076'',7813	1069'',4180
1805 to 1822	1073'',4868	1076'',7782	1069'',4149

The values of the mean daily motion for each perihelion concluded from three periods, differ from each other by 0'',0035, 0'',0031, 0'',0004; on comparing them, two by two, we find in this motion an augmentation in each revolution of 0″,00063, answering to a diminution of 0,00106 in the time of revolution. But the difference is above being of the order of quantities that are neglected in calculating the effect of disturbing forces, this retardation, if it exist at all, would not be perceptible till after a great number of revolutions.

It is now intended from the passages observed in 1805 and 1819 to ascertain that of 1822. According to the preceding observations, we have in relation to that period,

$$n' = 1076'',7778 \text{; therefore,}$$

$$T' = \frac{360^\circ + 9858'',12}{n'} = 1203',591 + 9',155 = 1212',746;$$

the observations give 1212′,742. We can hardly expect greater precision in the result.

An important object is to determine the instant of the next return of the comet to its perihelion. In this new revolution, as well as in that which follows, the effect of the disturbing force is very inconsiderable, and the elements undergo very little variation. We may hope then to assign with certainty, those parts of the heavens which the comet will occupy on its reappearance.

Take for n'' the mean value 1069″,4158 which results from comparing the three observed passages; we have

$$T'' = \frac{360^\circ - 306'',61}{n''} = 1211',8766 - 0',2867$$
$$= 1211',5899;$$

this interval from May 24,4944, 1822,* the epoch of the passage to the perihelion, answers to Sept. 17, 0843, 1825, which will be the instant of its next return to that point.

As it may happen that from the configuration of the sun, the earth, and the comet about the time of this passage, the latter may perhaps be invisible, we will continue to apply the effects of disturbing forces in order to determine the instant of its second return to its perihelion in 1829.

The mean value of n'' being 1069″,4158, we may conclude

$$n''' = 1069'',4158 + 0'',6708 = 1070'',0866;$$

and consequently,

$$T''' = \frac{360^\circ + 712'',15}{n'''} = 1211',1170 + 0',6655$$
$$= 1211',7825;$$

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* The periods of absolute time in this notice are in mean time, reckoning from midnight meridian of Paris.

is the passage to the perihelion will take the 10, 8668, Jan. 1829, and we shall have

$$n'''' = 1070'',0866 - 0''5406 = 1069'',5460.$$

As to elements of the orbit, the following table shews the changes made in them in different periods.

Periods.	Alteration of Eccentricity.	Alteration of the longitude of the Perihelion.	Alteration of longitude of the Node.	Alter. of inclination of the Orbit.
1805 to 1819	+0,0019950	+5′, 1″,0	— 1′54″1	+ 2′ 48″8
1819 1822	—0,0039038	+9, 41,0	—10,50 0	—16 8 0
1822 1825	+0,0004305	+0, 15,2	— 0,10 1	+ 1 4 4
1825 1829	—0,0002920	+1, 19,1	— 0,39 2	— 0 54 6

By adding to the alterations in longitude of the perihelion and node 11′0″,4 for the first period, and 0′46″,3 for the others we shall have these alterations in relation to the moveable equinox.

By means of the preceding results, with the elements of 1822, corrected after the observations at Paramatta, the following table, for the times of passing to five perihelia has been formed.

Passage to Perihelion.	Eccentrick.	Longitude of Perihelion.	Longitude of Node.	Inclination of Orbit.	Mean daily Motion.	Semi major axis.
1805 Nov. 22,006	0,8464567	156°43′0″	334°18′29″	13°35′44	1073″4877	2,218912
1819 Jan. 27,752	0,8484517	156 59 1	334 27 36	13 38 33	1076 7791	2.214388
1822 May 24,494	0,8445479	157 11 29	334 19 32	13 22 25	1069 4158	2,224542
1825 Sept. 17,084	0,8449784	157 14 30	334 22 8	13 23 29	1070 0866	2,223611
1829 Jan. 10,867	0,8446862	157 18 35	334 24 15	13 22 34	1069 5460	2,224360

These are ephemerides to facilitate the search for the comet upon its returns. In 1825, the elongation of the comet from the sun, in the interval which the ephemeris includes, varies from 49 to 33 degrees, and accordingly we can hardly expect that it will be visible to us. And after it has passed to its perihelion, the circumstances are still less favourable. As to its return in 1828, the comet will be visible throughout Europe.

	Right ascension.	North'rn declination.	Distance of the Comet from the Earth.	Distance of the Comet from the Sun.	Intensity of Light.
1825. July 14,075	59°6′	28°47	1,775	1,345	0,175
24,073	70 5	30 52	1,597	1,198	0,273
Aug. 3,086	84 15	23 56	1,439	1,041	0,445
13,087	99 48	23 44	1,316	0,874	0,597
23,061	118 5	21 28	1,243	0,697	0,647
1828. Aug. 28,159	26°35′	23 17	1,487	2,175	0,096
Sept. 7,175	25 47	24 58	1,276	2,073	0,143
17,161	23 30	26 29	1,089	1,967	0,218
27,183	19 24	27 50	0,920	1,855	0,344
Oct. 7,191	12 57	28 43	0,774	1,737	0,552
17,162	3 52	28 36	0,658	1,614	0.886
27,157	352 33	26 51	0,574	1,483	1,330
Nov. 6,173	340 23	23 13	0,521	1,345	2,033
16,171	329 1	18 13	0,494	1,198	2,851

Report made to the Academy of Sciences. By MM. Laplace, Legendre, and Poisson, upon a Memoir of M. Damoiseau relative to the Disturbances in the Motion of the Comet of 1819, *for a short period.*

Till lately there had been but one comet whose rereturn to its perihelion has been observed, and whose periodical time is known. This is the comet of 1682, which occupies about 76 years in performing its whole revolution. Halley discovered it to be the same with one which had already been observed, at two other epochs, in 1531, and 1607, and he predicted its return in 1758 or 1759. At this latter epoch, and before the comet had become visible, Clairaut calculated the instant of its passing its perihelion, having regard to the disturbances in its elliptical motion. The result of that immense labor differs only a few days, as is well known, from the time of the observed passage, which took place on the 12th of March, 1759. This remarkable phenomenon, of the reappearance of a comet at its perihelion, after one or more revolutions, has also occurred in our time with respect to the comet discovered in 1818, by M. Pons, who at that time observed it at Marseilles. The comparison of its parabolic elements with those of a comet observed in 1805 leads to the suspicion that the two bodies are the same. The remark was made at the Bureau des Longitudes, by M. Arago, when the discovery of the new comet was there announced;

it was also made in Germany by M. Olbers, who thought moreover, that this comet must be the same with that which had been observed in 1795 and 1786. According to this conjecture, its time of revolution can be only a very short period, and it is indispensably necessary to have regard to the ellipticity of its orbit in the calculation of its elements; this is done by M. Encke, astronomer at Gotha. The elliptic elements which he determined, either from the observations of 1805 or those of 1818 differ from each other still less than the parabolic elements, and there is no longer any doubt that they belong to the same body, the periodic time being nearly three years and a half. It has performed ed since its first appearance in 1805, four entire revolutions, and returned to its perihelion at the commencement of 1819. If we regard only the shortness of its successive revolutions this body might be considered a planet; but it has continued to be classed among comets, on account of the appearance it presents, and its not being visible to us in all parts of its orbit. To facilitate the observation of its return in 1822, M. Encke proposed to calculate an ephemeris relative to that epoch, but as the comet was for a great part of this new revolution at a very small distance from Jupiter, he was obliged to calculate the perturbations produced by the disturbing force of that planet, and he found in fact that in consequence of this principal action, the time of anomalistic revolution, the mean duration of which had been about 1204 days, from 1805 to 1819 would be augmented by about nine days in the following revolution. He announced that in 1822 the comet, in consequence of its declination, would not be visible in Europe, and that, in order to observe it, it would be necessary to go into the southern hemisphere. The event has completely verified this prediction. The comet returned to its perihelion in the latter part of May, 1822. It was observed at Paramatta in New Holland, by M. Dunlop, who first recognised it, and by Gen. Brisbane, and by M. Rumker, who determined its positions. The elliptic elements, deduced from this re-appearance agree in the most satisfactory manner with those calculated by M. Encke before the appearance of the comet, according to former observations, making allowances for irregularities produced by disturbing forces. Although this important calculation of M. Encke was well known the Academy which awarded to him, three years ago, the astronomical prize, founded by La Lande, we have thought it proper to mention the principal results, before we make known what M. Damoiseau has added, in the memoir we have submitted to your examination.

The first part of this memoir was presented to the Academy the 16th of February last, and the second part the 26th of April. In the first part, M. Damoiseau, speaking of the elliptic elements calculated by M. Encke for the epoch of 1819, determines the perturbations which they have undergone from that epoch to 1822. The elements which he has deduced for this last epoch differ but little from those of M. Encke, which we have already cited; and it could not be otherwise, because, having both adopted the same data, the difference in their results must be owing to the difference in the methods they pursued. The duration of the anomalistic revolution of 1819 and 1822 being nevertheless known by observation, M. Damoiseau has made use of it in order to rectify the daily motion of 1819, of which he had already availed himself. He decided at the same time the mean daily motion of 1822, which was necessary to enable him to calculate the perturbations of the comet, from its first appearance to its next return to its perihelion. In this new calculation, M. Damoiseau makes use of the elements of 1822, deduced from the observations at Paramatta. The perturbations of this revolution are much less considerable than those that happened before, and the duration between two consecutive passages through its perihelion, will be about 1212 days like the preceding. M. Damoiseau fixes the next return of the comet to its perihelion, on Sept. 17, 1825. He gives for that epoch its elliptic elements, and he assigns the places which it will occupy in the heavens during the time of its appearance, but thinking that it will then be too near the sun to admit of observations being taken, he has proposed to calculate the epoch of its following return, and its ephemeris for that epoch. These calculations are the subject of M. Damoiseau's second memoir. The second return to the perihelion which he has determined, will take place Jan. 10, 1829, and the author announces that the comet, according to his ephemeris will be visible in every part of Europe. The time between this passage through the perihelion, and that of 1825 will be very nearly 1211 days, or only one day less than the period of the preceding revolution.

The second part of the memoir of which we have been speaking, contains, also the calculation of the perturbations of the comet from its appearance in 1805, to its return in 1819. Thus the entire interval of twenty-four years, which the author has embraced in his calculations, comprehends three observed passages through the perihelion, and the two nearest returns to that point. The comparison of the three observed passages furnishes, for the epoch of each one of them, three values of mean daily motion, which differ but little from each other; the author has concluded for the last appearance upon the mean of these three values, by uniting with this the other elliptic elements calculated according to the observations at Paramatta; he has formed a table, which may be regarded as containing the definitive results of all his labours, and in which he has collected the elliptic elements answering to five passages through the perihelion, comprehended in the twenty four years which he had been considering. If we compare these elements, relative to the years 1805 and 1819, with those which are derived from actual observation made at those epochs, it will be seen that they agree in such a manner that the greatest deviations are but one minute in the longitude of the perihelion, five in that of the node, and two in the inclination of the orbit; but this comparison is not the best means of ascertaining the degree of exactness in the results obtained by M. Damoiseau. To judge of it properly it will be necessary that the author should calculate according to the elements, the places of the comet in these two epochs, in order to compare them with the places which have been directly observed.

M. Damoiseau has followed, in the calculation of the perturbations, the known method which consists in dividing the entire curve described by the comet, into portions, for each one of which we determine the effect produced by the disturbing forces of the planets. In the first part of his memoir, these portions answer to angles of 10° of eccentric anomaly. In the second part he has preferred to take intervals of time, nearly equal to each other, and each about thirty days; but in both cases, he is confident that he can, without any sensible error, observe in each portion of the orbit the variations of the mean motion; as proportional to the square of the time, and those of the other elements as proportional to its first power. However, we should observe that, if it were necessary to have regard to higher powers of this variable quantity, that the formulas which the author has given for this purpose, in the beginning of his memoir, would not be exact, and that it would be necessary to recur to the ordinary formulas of quadratures. Although the interval of time comprehended between the appearance of 1805 and that of 1819, comprises four revolutions of the comet, M. Damoiseau, in calculating the disturbances which have taken place in the course of that period, has always retained the same values of the primitive elements. It would nevertheless have been proproper to change them, at least every revolution, taking into consideration each time the preceding disturbances. This is a point which it will be sufficient merely for us to mention to the author if he proposes to review and complete his work.

In the period of twenty four years which the author has observed, he has constantly computed the action of Jupiter, and the action of the Earth and Venus only in that part of the period in which the comet is so near that this action is perceptible. As to the other planets, he has thought he might neglect their action, which he has regarded as imperceptible on account of their distance, or the smallness of their size. He thought moreover that he had no occasion to extend his caculations to the epochs of ~~1795~~ and 1786, in which the same comet had been observed, because he considered those observations not very exact, or not sufficient to determine the elements of the orbit. M. Encke on the contrary was desirous to represent these early observations; but he discovered that in the observed right ascensions and declinations, it would be necessary to suppose errors, the mean of which would amount to twenty four minutes, and this he judged to be inadmissible. To explain these differences between the results of calculation and observation, M. Encke thought that it would be necessary to recur to the resistance of ether, and he has announced in a late number of M. de Zach's Astronomical Correspondence, that by taking into account this tangential force, and making a suitable supposition in relation to its intensity, these great differences might be made to disappear, or at least be so far reduced as to be attributable to errors in the observations. There is no doubt that if the celestial bodies moved in a resisting medium, the effect of that resistance would be more sensible in the motion of comets than in that of the planets, as at the surface of the earth, we perceive that the resistance of the air has most effect upon the motion of those bodies having weight, whose density is least, and it actually is known that the density of comets is incomparably less than that of the planets; but if the comet, which has been the subject of consideration is destined to exhibit to us in the

the heavens the effects of a force, of the necessity of which no other phenomenon has ever yet made us sensible, it must require a greater number of revolutions of that body to establish with any probability so delicate a point in astronomical science.

The calculations, to which M. Damoiseau has devoted himself, with a zeal worthy of all commendation, have already given with a great degree of exactness, the motion of the comet of 1819. The same philosopher has likewise calculated the next return of the comet of 1759 to its perihelion, and the academy of Turin has rewarded him for this laborious task by decreeing him the prize which had been offered on that subject. Yet the importance of these questions has made it appear desirable to you that the theories of these two comets should be made more perfect, and in proposing, at your last meeting, the general theory of the perturbations of comets as the subject of the prize of 1826, you have mentioned as particular applications, the motion of these two comets, so different from each other in the duration of their revolutions. We unite with you in the hope that geometers and astronomers, not connected with the Academy, will answer to your appeal, but in the mean time we must not do the less justice to the researches of M. Damoiseau. We think that they decree your approbation, and we would propose to you to direct that the Memoir of which we have given you an account, be printed in the *Recueil des Savans étrangers.* This report and its conclusions were approved by the Academy in the session of 24th May, 1824.

Art. XV.—*On the Influence of Solar Light on the Process of Combustion.* By Thomas M'Keever, M. D. (Abridged for this Journal from the Ann. Phil. by H. D. Ward.)

The popular opinion that the sun's rays falling directly upon a common fire, have the effect of retarding the progress of combustion, induced Dr. M'Keever to institute several experiments, with a view to determine its accuracy. And we are happy to find another proof of the position, that opinions generally entertained among men, do commonly rest more or less on experience and observation.

Exper. 1.—Two portions of green wax taper, each weighing ten grains, were both ignited at the same moment. One was placed in a darkened room, the other exposed to broad sunshine in the open air. Thermometer in the sun, 78° Fahr.; in the room 67°.

In five minutes that placed in the sunshine lost 8½
Darkened room, - - - - - - - - - - - 9¼*

Exper. 3.—A common mould candle, fourteen inches in length and three in circumference, was accurately divided into inches, half inches and eighths, and exposed in the first instance to strong sunshine. Thermometer 89° Fahr.; atmosphere remarkably calm:—to consume one inch it took 59′0″
In darkened room, (Temp. 68° F.), - - - 56′0″
In ordinary daylight, (Temp. 68° F.), - - 57′10″

Exper. 5.—Two lanterns, one coated with black paint and the other naked, were each furnished with a portion of taper of precisely equal weights, and both exposed to the full glare of sunshine. In ten minutes

that in painted lantern lost 16½ grains.
that in uncoated lantern, - 15 grains.

Dr M'Keever, by varying the last experiment, sought also the results of combustion in the full light of the moon, but could detect do difference between the combustion of the tapers in the different lanterns, whether in moonlight or in a darkened room.

In reasoning upon these results, says Dr M'Keever, it occurred to me that they were probably owing to the well known decomposing power possessed by the solar rays, in consequence of which the shell of air that immediately encircles a particle of matter about to enter into combustion, is deprived, to a certain extent, of its oxygenous principle; and is thus rendered less fitted for the maintenance of this important process.

As the solar rays are now ascertained to consist of three distinct species of radiant matter, viz. those that impart heat, those that impart light, and the chemically acting rays, it appeared to me that the best mode of putting this conjecture to the test of experiment, would be to try whether any difference could be detected in the loss sustained by a lighted taper when exposed to the several portions of the solar spectrum.

I allowed the rays of one colour only to pass at a time through an opening made in a piece of pasteboard, the room

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[*] Experiments two and four only confirm the general result, and are omitted in this abstract.

being darkened, and the prism operating by means of an opening in the window shutter. A piece of green taper accurately marked after the usual manner, was now ignited, and submitted to different portions of the spectrum, with the following results. To consume two inches of taper in the

red ray, it took	8′ 0
green ray,	8′ 20″
violet ray,	8′ 39″
verge of violet ray,	8′ 57″

Commencing with the violet ray, it took to consume one inch at the

verge of the violet ray.	4′ 36″
in the centre of the violet ray,	4′ 26″
green ray,	4′ 20″
red ray,	4′ 16″

This short enquiry then, although perhaps, not conducted with all that delicacy and precision so necessary for strict philosophical research, would apppear to warrant two conclusions of some interest. First, that the solar rays, in proportion to their intensity, are possessed of the power of retarding, to a considerable degree, the process of combustion; and that consequently, the popular ideas on this subject are founded in truth. Secondly, that this phenomenon is occasioned by the action of the chemical rays on the portion of the atmospheric air, that immediately envelopes a particle of matter about to enter into a state of combustion, aided no doubt by the high temperature of the portion of materials that have already commenced this process.

ART. XVI.—*An attempt to establish the first Principles of Chemistry by Experiment.* By THOMAS THOMSON, M. D. F. R. S. Regius Professor of Chemistry in the University of Glasgow, 2 Vols. 8vo. Lond. 1825. [W.]

WHILE chemistry invites the philosopher to explore the wonders of nature, and unfold the laws regulating its mysterious processes, it holds out inducements, less ennobling it is true, but not less powerful, to the artist and manufacturer, in the hope it inspires of increasing his pecuniary advantages. Besides the number of general students it has drawn from the retreats of classical literature, and the abstruser sciences, a crowd of votaries have appeared from the more busy walks of life. It has created a new population of scientific artists and practical philosophers, who are to be sought in the workshop and in the manufactory.

In those countries where the arts have attained a high degree of perfection, there are probably few men who aim at precision in their processes, that do not become students of chemistry, and who do not follow with lively interest, every discovery in that science which may in any way affect their peculiar occupations. The multitude of scientific journals, elements, dictionaries, and systems of chemistry which the press is continually pouring forth, and to which a valuable addition has been made in the work of Dr Thomson, indicate the spirit which is abroad, and the increassing multitude of scientific readers. Although a difference of opinion may exist among chemists, in regard to some of the views of Dr Thomson, we cannot but think they will agree in considering this as the most elaborate and original work on chemical philosophy which has appeared for many years.

Dr Thomson's volumes abound in new, ingenious, and delicate experiments, the object of which, was to determine the atomic weight of bodies; or, in the language of the author, to reduce the whole doctrine of atoms to the utmost degree of simplicity and accuracy, so as to put it in the power of the chemical manufacturers in general, to avail themselves of it in order to bring their processes to the requisite degree of precision.

Whatever degree of hypothesis may have been blended with the discussions and views in which the atomic theory is concerned, it has now attained a value and importance far beyond what its early advocates could have anticipated. Among other advantages, it is conceived by Dr Thomson, that a knowledge of the atomic weights of bodies, and of the weights of the integral particles of the salts, will be found highly useful to medical practitioners. It will enable them "to avoid some very awkward blunders, into which too many, even of considerable celebrity, frequently fall, to the no small inconvenience of their patients. Among chemists too, remarkable examples could be pointed out, of men who would take it amiss if the world did not give them credit for possessing much chemical science, falling into mistakes, that no student who had followed and understood the lectures on chemistry, delivered at the College of Glasgow, would run any hazard of committing."

The publication of the present work has shown that Dr Thomson has not been content with the faithful performance of the prescribed duties of his office, but with indefatigable zeal, has devoted no small portion of his "leisure" hours, to private and laborious investigation. The results which are contained in these volumes, will speedily become known wherever science is cultivated, and redound not less to the honor of the University, than to that of its distinguished professor.

Dr Thomson's work contains the results of many thousand experiments, which have occupied the whole of his time, except what was necessarily devoted to the duties of his situation, for the last five years ; an appropriation of time which may, to those unacquainted with the subject, seem unnecessarily large ; but which is fully warranted by the importance of the subject, and the necessity of obtaining accurate results. Those who are not practically conversant with the minute details of experiments like these, and who have not watched the progress of chemical science, can have but very imperfect notions of the degree of accuracy and precision, which its processes have acquired, and of the amount of time they consume. The processes of chemistry have arrived within a few years, at such a degree of precision and accuracy, and so much care is now bestowed upon experiments, that we may rely upon the results within less than one per cent. I believe, indeed, says Dr Thomson, that the error in many of Berzelius' analyses does not exceed one thousandth part.

As a specimen of the manner in which the experiments of Dr Thomson were conducted, we shall select a portion of the section relating to the composition of zinc. As it had been demonstrated by Gay-Lussac and Humboldt, that water is a compound of oxygen and hydrogen gases, in the proportion of one volume of the former to two volumes of the latter, it occurred to Dr Thomson that by a very careful analysis of water, the relation between the volumes and the weights of these gases might be determined. These experiments were made by means of zinc, and it therefore became highly important to know the exact composition of the oxide of that metal. The zinc of commerce is never perfectly free from carbon, and small quantities of other metals ; that

The zinc employed by Dr. Thomson "was obtained by exposing common zinc to a white heat in a stone-ware retort, luted to a receiver nearly filled with water. At this temperature, the zinc is sublimed, and freed from all its impurities, except a trace of cadmium too minute to occasion any sensible error in the result of the experiments. The zinc thus distilled over, should be melted in a crucible, and poured upon the surface of a clean, smooth sandstone. We thus obtain it in a thin sheet which can be easily broken into small pieces to suit the object in view.

21·25 grains of this zinc were introduced into a small green glass retort, the weight of which had been previously determined. As much very weak but pure nitric acid as was fully sufficient to dissolve the whole zinc was introduced into the retort. As soon as the metal was all dissolved, a receiver was applied to the retort which was placed in hot sand, and allowed to remain till the whole liquid contents had distilled over. The liquid in the receiver was saturated with carbonate of ammonia, and boiled for some time to see whether it contained any zinc; but no precipitate appeared.

The retort, thus freed from its liquid contents, was placed upon a charcoal fire, and kept for about an hour in a dull red heat. This completely decomposed the nitrate of zinc, which existed at first in the retort, dissipated the nitric acid, and left nothing behind but the 21·25 grains of zinc, now converted into an oxide. Being placed in the scales, the zinc, by being oxydized, was found to have increased in weight 5 grains. Thus, it appears that oxide of zinc is composed of

Zinc	21,25
Oxygen	5
	26,25

or, dividing each constituent by 5 to bring them to their lowest terms, of

Zinc	4,25
Oxygen	1
	5,25

As zinc combines with oxygen in only one proportion, it is reasonable to conclude that the oxide is a compound of 1 atom zinc and 1 atom oxygen. If this supposition be admitted, the atom of zinc is obviously 4¼ times heavier than the atom of oxygen. And if we represent the atomic weight of oxygen by unity, then the atomic weight of zinc will be 4·25, and the integrant particle of ozide of zinc will weigh 5·25; or in other words, 5·25 of oxide of zinc will just saturate an integrant particle of any acid with which it is capable of combining. The law holds with all acids; but I shall at present verify it only by referring to three acids; namely, the sulphuric, carbonic, and arsenic acids. It will be shown in the 5th, 1st, and 7th sections of the sixth chapter of this work, that the atomic weights of these acids are as follows:

Sulphuric acid	5
Carbonic acid	2,75
Arsenic acid	7,75

Now, 5·25 of oxide of zinc will just saturate 5 of sulphuric acid, 2.75 of carbonic acid, and 7·75 of arsenic acid; or in other words, the sulphate, carbonate, and arseniate of zinc, supposing them anhydrous, are composed as follows:

1. SULPHATE OF ZINC.

Sulphuric acid	5
Oxide of zinc	5,25

2. CARBONATE OF ZINC.

Carbonic acid	2,75
Oxide of zinc	5,25

3. ARSENIATE of ZINC.

Arsenic acid	7,75
Oxide of zinc	5,25

We shall verify this statement by a rigid analysis of these three salts.

1. SULPHATE OF ZINC.

Sulphate of zinc, or white vitriol, is a well known salt which crystallizes in beautifully transparent four-sided prisms, terminated by four-sided pyramids; or if the crystallization be rapid, it assumes the form of long slender white needles. However carefully prepared, it always reddens vegetable blues. But it crystallizes also with an excess of acid. In that case, the crystals assume the form of large flat rhomboids, which are not altered by exposure to the air, and may be raised to the temperature of at least 500° without being deprived of their excess of acid.*

18·125 grains of the neutral sulphate of zinc were dissolved in distilled water, and precipitated by a solution of carbonate of soda. The precipitate was separated from the liquid by means of a double filter, consisting of two pieces of unsized and well washed printing paper, made previously of the same weight. The carbonate of zinc on the filter was washed with distilled water, till the liquid which passed through ceased to render mu-

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* I am doubtful whether this salt constitutes a truly distinct species. I found its composition.

1⅓ atom acid	6,66
1 atom oxide	5,25
8 atoms water	9,00
	20,91

or, it consists of one atom of the potrohydrate of sulphuric acid, combined with three integrant particles of the neutral salt.

riate of barytes muddy. The filter was suspended in a linen bag above the sand bath, in a temperature of 85°, till it was apparently dry; and it was finally left for some time in a temperature of about 212°. It was afterwards weighed by placing the two filters in opposite scales, and adding weights till the two scales were exactly counter poised. The weight of the carbonate of zinc was exactly 8 grains. It was put into a platinum crucible, which was covered with a lid, and exposed for half an hour to a red heat. There now remained 5·25 grains of oxide of zinc.

Thus it appears, that 18·125 grains of sulphate of zinc, in crystals, contain 5.25 grains of oxide of zinc. It is still easier to prove that the sulphuric acid contained in this quantity of the salt weighs precisely 5 grains.

But it will be necessary to state, in the first place, that when 15·5 grains of crystallized muriate of barytes are exposed to a red heat, they lose 2·25 grains of their weight, and are converted into 13·25 grains of chloride of barium. When 13·25 grains of chloride of barium are dissolved in water, the solution contains just 9·75 grains of barytes. Now, 9·75 represents the atomic weight of barytes, which requires for saturation exactly 5 of sulphuric acid. And 14·75 parts of sulphate of barytes contain just 5 parts of sulphuric acid.

A little distilled water was put into two small cylindrical glass jars, and 18·125 grains of crystallized sulphate of zinc were dissolved in the one of these, and 13.25 grains of chloride of barium in the other. The two liquids being mixed, a double decomposition took place, and sulphate of barytes precipitated in the state of a white powder. This precipitate being collected, washed, dried, and heated to redness, weighed 14.75 grains, and therefore contained exactly 5 grains of sulphuric acid. A few drops of the colourless liquid from which this sulphate had precipitated, were put into a watch glass, and a small quantity of muriate of barytes mixed with it; but it did not become in the least muddy or even opal coloured, showing that it contained no sensible quantity of sulphuric acid. For I find that 1 grain of glauber salt (containing 0.2469 grain of sulphuric acid) dissolved in 12,000 grains of water, is sensibly precipitated by muriate of barytes. Hence it is clear, that in the present experiment, the quantity of sulphuric acid remaining in the liquid could not amount to $\frac{1}{40502}$ of a grain. Indeed, as the quantity of liquid never amounted to 1000 grains, the sulphuric acid remaining in solution, could not be so much as $\frac{1}{486024}$ of a grain.

Another portion of this residual liquid being mixed with sulphate of soda, was not in the least altered, showing that the liquid contained no sensible portion of barytes. Thus it appears, that the barytes from 13.25 grains of chloride of barium, or 15.5

grains of muriate of barytes, just saturate the sulphuric acid in 18·125 grains of sulphate of zinc. Hence, this acid just weighs 5 grains.

We see therefore, that in neutral sulphate of zinc, 5.25 parts of the oxide are combined with 5 parts of the acid : which was the thing to be proved. These two constituents added together make 10.25, which subtracted from 18.125 leave 7.875 for the quantity of water of crystallization. It will appear afterwards that this is equivalent to 7 atoms of water. Sulphate of zinc in crystals, then, is composed of

1 atom sulphuric acid	5
1 atom oxide of zinc	5,25
7 atoms water	7,875
	18,125

So that 18.125 is the weight of an integrant particle of the crystallized salt. It was the knowledge of this that induced me to make choice of 18.125 grains of the salt. My first experiments were made upon a much larger quantity ; but I have often repeated them with 18.125 grains, and obtained the result just stated.

If the solution of neutral sulphate of zinc be concentrated too much, it deposites, while still hot, an opaque crust upon the bottom of the vessel. This crust is very white, not so soluble in water as the crystals, and has not the least appearance of crystallization. It is neutral, though it reddens vegetable blues like the preceding salt ; and being analysed in the same way as the crystallized sulphate, was found composed of

1 atom sulphuric acid	5
1 atom oxide of zinc	5,25
3 atoms water	3,375
	13,625

So that an integrant particle of it weighs 13.625, and it differs from the crystals in containing only 3 atoms of water, instead of seven."

In the introductory part of the present work, Dr Thomson has given an historical sketch of the developement and progress of what has been termed the Atomic Theory ; of this we shall now offer an abridgment with additional remarks, from Beudant and the papers of other writers scattered through the various scientific journals.

From the time that bodies were conceived to be constituted of simple elements, it was believed that those elements were combined in certain proportions. The first accurate experiments on the composition of one class of bodies, viz. the salts, were made by Wenzel, a German chemist who published a small work in the year 1777, entitled *Theory of the Affinities of two Bodies*. He had observed that when two neutral salts mutually decompose each other, the resulting compounds are also neutral; and that the relative proportions of alkalies and earths which saturate a given quantity of the same acid, are the same for all other acids. If nitrate of lime for example, be decomposed by sulphate of potash, the nitrate of potash and sulphate of lime which result, preserve their neutrality, for the relative quantities of potash and lime which saturate a given weight of water and sulphuric acid are the same.

Bergman also bestowed much attention upon the determination of chemical proportions, and explained the phenomena noticed by Wenzel in the same way. Berthollet subsequently endeavoured to prove that the elements of bodies have certain fixed points, a maximum and minimum, beyond which they will not enter into combination, but that between these limits they unite in any proportion. The phenomena noticed by Wenzel were likewise observed by Richter, who endeavoured to determine the relative capacity of saturation of the acids and bases, and attached a number to each indicating this capacity. The little attention which the labours of this chemist attracted, is ascribed by Berzelius to two causes, viz. 1. The great inaccuracy of Richter's experiments. 2. The violent discussion between the antiphlogistic and phlogistic system, which originated about the year 1787.

The opinions of Bergman were contested by Proust, who demonstrated that the combinations which metals form with oxygen and with sulphur, are in fixed and invariable proportions, and that the supposed intermediate compounds are merely mixtures of two definite oxides or sulphurets.

In the year 1789, Mr Higgins published his "Comparative view of the Phlogistic and Antiphlogistic Theories," in which he maintains that when gases combine in more than one proportion, all the proportions of the same element are equal. This idea was founded on the corpuscular hypothesis, that bodies combine particle with particle, or with one, with two, or three, or a greater number of particles."* He does not appear, however, to have prosecuted the enquiry, but it was left for the genius of Dalton to give a much more general view of the subject, which has been usually called the atomic theory. Dr Thomson conceives that the views upon which this theory is founded, occurred to Mr Dalton in consequence "of his own experimental investigations," an opinion which though not subscribed to by all chemists, yet derives some support from the remark of Sir H. Davy, that Mr Dalton had apparently no knowledge of what Mr Higgins had written.* The account which Dr Thomson gives of his first knowledge of the views of Mr Dalton is as follows.—"I first visited him at Manchester on the 26th of August, 1804, and on that day he explained to me his notions respecting the composition of bodies. The ultimate particles of all simple bodies, are in his opinion *atoms* incapable of further division. These atoms (which Mr Dalton supposes to be surrounded by atmospheres of heat,) are all spheres, and are all of them possessed of particular weights which may be denoted by numbers. And he represented the atoms of the simple bodies by symbols."

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* Davy's Elements.

In 1807, Dr Thomson, published a sketch of this hypothesis in the third edition of his system of chemistry. In 1808, Mr Dalton published the first volume of his new system of chemical philosophy. The second volume appeared in 1810, in which he examined the combinations of oxygen with hydrogen, azote, carbon, sulphur and phosphorus; and various other combinations which he showed to be in accordance with the atomic theory. The third volume of this important work, Dr Thomson remarks has been printed for several years, but the author has thought proper to withhold it from the public.

The views of Mr Dalton have received great support from the experiments of Dr Wollaston and others. Berzelius has remarked that this discovery may be considered without exaggeration, as one of the greatest steps that chemistry has ever made towards perfection, and that the honour of the invention of the doctrine of *multiple proportions* is due to Dalton alone.

In order to express the relative weights of the atoms of bodies, it was found necessary to assume the atom of some body as a standard or unity. Mr Dalton made choice of the atom of hydrogen, and in this he has been followed by Davy, Brande, Dr Henry, and various other chemical writers. Other chemists have assumed oxygen as the unit, and among these is Dr Thomson, who assigns the following reasons.

"It is obvious, that the ratios are much more easily observed,

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* Elements.

and much more easily employed, the smaller the numbers are, by which they are denoted. Thus the ratio of 39841 to 116223 is the same as that of 1 to 3. But how very inconvenient would the great number of figures be, compared with the simple number 1 : 3. It is therefore highly important in chemistry, that the atomic weights of bodies should be represented by the smallest possible numbers. Now, when we make the atom of oxygen unity, the atomic weights of all bodies are represented by numbers eight times less than they would be if the atom of hydrogen be unity. It has been represented by some persons, in favour of the atom of hydrogen being unity, that in that case the atomic weight of every body is a whole number; whereas, when oxygen is the unity, fractions are frequently unavoidable. And these fractions, it is said, render the atomic weights more complicated and unmanageable, than the greater size of the whole numbers deduced from hydrogen as a unity. But this view of the subject is quite erroneous. It will be shown in this work, that even when oxygen is made unity, nearly one half of all the atomic weights of bodies are whole numbers. And when fractions exist, they are always either 0.25, 0·5, or 0.75; except in four or five cases, when the fraction is 0·125, 0·375, or 0·625. If hydrogen be unity the atom of manganese is 28, while it is 3½ if oxygen be unity. The atom of barytes on the former supposition is 78, on the latter 9¾. Now surely it will not be said that the fractional numbers are more unwieldy, or more unmanageable than the whole numbers; while in all cases of whole numbers the advantage on the side of the latter method is very great. Thus, if hydrogen be unity, the atom of uranium is 208, while if oxygen be unity it is only 26."

The next step in the progress of the atomic theory is to be found in the researches of Gay-Lussac and Humboldt, who found that one volume of oxygen gas combines with two volumes of hydrogen gas to form water. The first of these two philosophers some time after, found that gases in general unite with each other in a very simple manner; one volume of one gas, either combining with one volume, with two, or with half a volume of the other; or in other words, that gases either combine in equal volumes, or the volume of one gas is a multiple by a whole number of that of the other. It has been remarked by an eminent chemist, that if we substitute the word *atom* for that of *volume*, and imagine the substance to be in the solid instead of the gaseous state, Gay-Lussac's theory coincides exactly with Dalton's, and is a direct confirmation of its truth. But, says Dr Thomson, Mr Dalton, instead of welcoming the papers which contained the discovery of Gay-Lussac, as an important addition to his own views, set himself to show that Gay-Lussac's opinions were ill founded. The subsequent researches of chemists, however, "have left no doubt about their accuracy; and if Mr Dalton still withholds his assent, he is, I believe, the only living chemist who does so."

Dr Thomson, at page 19, has taken a very interesting notice of Professor Berzelius, of Stockholm, who has done so much for the establishment of the atomic theory. To him are we indebted for a far greater number of accurate analyses, than to the whole chemical world put together. This indefatigable chemist has embraced every department of chemistry with equal ardour, and to such a degree of accuracy has he attained, that as already remarked, it is believed the error in many of his analyses does not exceed one thousandth part.

As the views of Berzelius are peculiarly interesting, we shall avail ourselves of the notice lately taken of them by Mr Children,* reserving any further account of Dr Thomson's work for a future number.

When we reflect on the cause of chemical proportions, the most probable idea that presents itself to our imagination is, that all bodies are composed of elementary particles or atoms, incapable of mechanical division,† and which unite together in such a manner, that an atom of one element combines with 1, 2, 3, &c. atoms of another element. With this simple postulate, to which the mind readily accedes, it is easy to explain all the phenomena of chemical proportions. By the union of two or more elementary atoms, compound atoms are formed which are as incapable of mechanical division as the former, and these again unite together to form still more compound atoms, and so on to the most compound. Berzelius divides these several combinations into separate orders—the simplest, or the atoms of the first order, are composed of simple elementary atoms, and are either inorganic, or organic. The former contains only two elements, the latter always three at least. Compound atoms of the second order are produced by the union of compound atoms of the first order; those of the third by the union of atoms of the second order, &c. Thus sulphuric acid, potash, and water, are compound atoms of the first order, since they consist only of a base and oxygen. Sulphate of potash and sulphate of alumina are atoms of the second order, being formed by the union of compound atoms of the first order. Anhydrous alum, which is composed of the two last mentioned salts, belongs to the third order; and crystalized alum, which besides those elements, also contains several atoms of water, is an instance of a compound of the fourth order.

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* Annals of Philosophy.

† On the subject of the limited divisibility of matter, see Dr Wollaston's beautiful paper, "On the Finite Extent of the Atmosphere."—(Phil. Trans. for 1822, pp. 39.)

"But taking it for granted that bodies are composed of indivisible atoms, certain laws must regulate their combinations, in order to establish them in definite chemical proportions, and on which the constancy of those proportions must depend. The existence of such laws and the probable manner in which elementary atoms combine, is inferred from experiment, which teaches us that

"1. *An atom of one element combines with one, two, three, or more atoms of another element.* This is of most frequent occurrence, so that in the greater number of compound atoms, one of the elements enters only as a single atom.

"2. *Two atoms of one element combine with three atoms of another element.* This may happen whenever the quantity of oxygen in a protoxide is to that in the deutoxide as 1 : 1½; as is the case with iron. If the first oxide be composed of an atom of base united to an atom of oxygen, the second must contain two atoms of base united to three atoms of oxygen; for there can be no such thing as half an atom. This apparent anomaly may be reconciled in a different manner, by supposing that there may be another hitherto unknown oxide of iron, containing half the quantity of oxygen that is contained in the lowest of the two known oxides. According to this view, an atom of base in the unknown oxide is united to one atom of oxygen, and in the two known oxides to two and three atoms respectively. Dr Thomson with Berzelius adopts the former solution.

"In the compound atoms of the second order, the ratio of two atoms of one element to three of another is found less equivocally, though the instances are rare. Thus the hydrated red oxide of iron is composed of two atoms of oxide combined with three atoms of water; the subsulphate of copper contains two atoms of acid and three atoms of base. Unless we consider these as the true proportions of the elements of the above compounds, we must suppose the oxide of iron, as well as the sulphuric acid, to contain six atoms of oxygen; but till new facts show the probability of that supposition, there is sufficient ground for considering the ratio to be that of 2 : 3. There is no reason for supposing that two atoms of one element combine with four, five, six, or a greater number of atoms of another element, but in the varied productions of the mineral kingdom, we find compounds somewhat different from those we can produce in our laboratories. Amongst the silicates, combinations in which three compound atoms of the first order are united to four of the same order are frequent, as in laumonite, amphigene, &c.; but in our artificial productions, analogous cases are extremely rare.

"As a general conclusion, we may assume, that in inorganic compounds the simple atoms combine in very limited proportions; the most common is that of one atom of one element with one or more atoms of another element, so that in most compounds, one of the elements may be represented by unity: the next most common proportion is that of two atoms of one element to three of another; and in the mineral kingdom, in the compound atoms of the third and fourth orders, we sometimes meet with three atoms of one body united to four atoms of another body.

"There is another law which according to Berzelius, regulates the combination of compound atoms of the first order; namely, that oxidated bodies always combine in such proportions that the number of atoms of oxygen in one of them is a multiple by a whole number of the number of atoms of oxygen in the other; and in like manner, in the combinations of sulphuretted bodies, the sulphur in one is a multiple by a whole number of the sulphur in the other. Or, to express the law in general terms, compound atoms of the first order, having a common electro-negative element, combine in such proportion that the electro-negative element of one atom is always a multiple by a whole number of the electro-negative element of the other.

"The only known exceptions to this law are the acids of phosphorus, nitrogen and arsenic, which combine with other oxidated bodies in such proportion that the number of atoms of oxygen in the oxide is one or more *fifths* of the number of atoms of oxygen in the phosphoric, nitric, and arsenic acids, and one or two *thirds* of the same number in the phosphorus, nitrous and arsenious.

"When two salts, having a common acid but different bases combine, the number of atoms of oxygen in one of the bases is a multiple by a whole number of the number of atoms of oxygen in the other; and consequently, the acid in one of the salts is a multiple by a whole number of the acid in the other. In alum and feldspar, the number of atoms of oxygen in the alumina is triple the number of atoms of oxygen in the potash; and in like manner the quantity of sulphuric acid and of silica, respectively combined with the alumina, is triple that combined witth the potash. In the double tartrate of potash and soda, the two alkalies contain the same number of atoms of oxygen, and are consequently combined with the same number of atoms of tartaric acid.

"When two salts, having a common base but different acids combine, the number of atoms of oxygen of the portion of the base combined with one of the acids is a multiple by a whole number of the number of atoms of oxygen in the portion of base combined with the other acid; or, the number of atoms of oxygen in one of the salts is a multiple by a whole number of the atoms of oxygen in the other. We have an instance of this species of combination in Datholite, a compound of borate and silicate of lime, in which the lime is equally divided between the two acids; whilst in the blue carbonate of copper, the quantity of base combined with the carbonic acid is twice as great as that combined with the water.

"In organic bodies, the compound atoms of the first order contain at least three elements, oxygen, hydrogen, and carbon, and their atoms are capable of combining in all sorts of proportions, so that neither of them can be taken as unity with reference to the rest. But when organic atoms of the first order combine with compound inorganic atoms of the first order, as when a vegetable acid combines with an oxide, they follow the same laws that regulate the combinations of the compound inorganic atoms, and the oxygen of the organic atom is a multiple, or sometimes a submultiple, by a whole number of the oxygen of the compound inorganic atom. Hence these combinations present the same phenomena of definite proportions, as those of inorganic nature, and it is only in the formation of organic atoms of the first order, that combination is possible in all sorts of proportions.

"In the earliest experiments with the voltaic pile, it was found that the common salt, a solution of which was usually employed to moisten the pasteboard discs interposed between each pair of metallic discs, became decomposed by its action; and about the year 1805, MM. Hissinger and Berzelius published their experiments on the galvanic decomposition of the muriates of ammonia and lime, and several other neutro-saline solutions.

"The results of those experiments demonstrated that when electricity traverses a saline solution, or any other liquid conductor, its elements are separated in such a manner, that some collect round the positive, and others round the negative pole; and that oxygen, acids, and oxidated bodies terminate to the former, whilst combustible bodies, alkalies and the earths, find their point of rest at the latter.

"On the 20th of November, 1806, Sir Humphrey (then Mr) Davy, read his Bakerian lecture on some chemical agencies of electricity, before the Royal Society, which was afterwards published in the Philosophical Transactions for the following year. In this celebrated paper which obtained the prize offered by Napoleon Bonaparte, for the best essay on voltaic electricity, Sir Humphrey Davy clearly promulgated, for the first time, the laws by which the chemical agencies of electricity are regulated, and the principles on which their power of suspending or destroying the usual order of chemical affinities depends. He showed that different elementary substances have different electrical energies, some being naturally positive, and others negative, with respect to each other; and that when a compound formed of two such elements, is decomposed by the voltaic battery, the body possessing positive energy is repelled by positively electrified surfaces, and attracted by negatively electrical surfaces; and that the body possessing the negative energy follows the contrary order.

"Adopting these views, Berzelius divides all substances into two great classes, the electro-positive and electro-negative. Simple bodies belonging to the former class, as well as their oxides, always assume the possitive state when they meet with other simple bodies or their oxides belonging to the latter; and the oxides of the first class bear the same relation to those of the second, that salifiable bases bear to acids. He considers oxygen as the most electro-negative of all bodies, and the only one whose electrical relations are invariable, it never being positive with respect to any other, and he places it accordingly at the head of his table exhibiting the supposed order of elementary substances with respect to their electrical relations. The last substance in the table, and consequently the most possitive, is potassium, and all the intermediate substances between oxygen and that body are considered as negative to all those which stand below them, and positive to all that stand above them in the table.

"Long before any idea had been formed of the electrical relations of simple combustible bodies, their oxides were divided into acids and bases, the first forming the electro-negative, the second the electro-positive class; amongst the bodies of the first class a weak acid often serves as base to a more powerful acid, and in the electro-positive series, a weak base frequently acts as an acid with respect to one more strongly electro-positive.

"The electrical relations of oxides usually depend on those of their bases; thus an oxide is electro-negative to another oxide, if the base of the former be negative with respect to that of the latter, and *vice versâ*. Sulphuric acid for instance, is electro-negative with respect to all the metallic oxides, because sulphur is negative with respect to all the metals; the oxides of potassium and zinc, on the contrary, are electro-positive in regard to all oxidated bodies, with respect to whose bases potassium and zinc are positive. Hence acidity does not depend, as the antiphlogistic theory assumes, on oxygen as the acidifying principle, but rather resides in the radical of the acid, and the oxygen enters indifferently into the most electro-positive and electro-negative bodies, or the strongest bases and acids.

"When the two electricities are separately manifested in any body, they are concentrated in two or more opposite points or poles, analogous to the poles of a magnetic needle. This electrical polarity of which the tourmaline furnishes a striking instance, must also belong to the minutest particles of the body, as well as to the whole mass; and this affords an easy solution for all the phenomena of electro-chemical affinity; for the different forces with which elementary bodies unite may be conceived to depend on the different intensities of the electrical polarities of their atoms. This hypothesis, however, is not sufficient to explain why some bodies are *electro-positive* and others *electro-negative*; but if we imagine the atoms of such bodies to possess *unipolarity*, similar to that which Ehrman observed in various substances, we may conceive that in the atoms of electro-positive bodies, the electricity of the positive pole predominates, and in those of electro-negative bodies, that of the negative pole, and consequently these bodies will always be in opposite states with respect to each other.

"The degree of affinity between different bodies cannot however depend wholly on their specific unipolarities, but rather on the intensity of the polarities generally; for oxygen and sulphur, both electro-negative substances, combine with much greater energy than oxygen and copper, although the latter is an electro-positive element; and the influence of temperature has a powerful effect in modifying attractions.

"If these conjectures," says Berzelius, "present a correct idea of the relations of bodies to electricity, it follows that what we call chemical affinity is nothing else than the effect of the electrical polarity of their atoms, and that electricity is the primary cause of all chemical action."

But it is time to quit this part of the subject, and turn to that which is more immediately the object of this abstract; and first, we shall endeavour to explain the manner in which the relative weights of the atoms of bodies are determined.

It is obviously necessary for this purpose, that some substance should be fixed upon, the weight of whose atom may be assumed as unity; Dalton chose hydrogen for his unit, as the substance of which the smallest weights enter into combination : He has been followed by Davy, Brande, Henry, Phillips, and various other writers ; whilst Wollaston, Thomson, and Berzelius adopt oxygen as their lowest number, that substance being of all others most universally present in inorganic bodies. On the scale of chemical equivalents, Dr Wollaston reckons oxygen as ten, Thomson considers it as one, and Berzelius as one hundred. It is of small consequence which atom be selected for the purpose, or what relative value be assigned to it, whether ten, one, or one hundred ; but whichever be chosen, the weights of the atoms of all other bodies must be exressed in some function of that unite.

[*To be continued.*]

ART. XVII. *Abstract of a Memoir accompanying Meteorological Observations made at Washington, D. C. in the years* 1823–4. By JULES DE WALLENSTEIN, Esq. Corresponding Member of the Academy of History of Madrid.* [*Translated and abridged for this Journal.*]

THE barometer which M. Wallenstein uses in his observations at Washington was constructed by M. Fortin, a skilful artist, who has enriched the Royal Observatory at Paris, with an excellent circle, and who was before known for the extreme exactness of his balances and other instruments which demand equal skill in their construction. The cistern of this barometer has a moveable bottom : an ivory point indicates the moment when the surface of the mercury in the cistern is at its true level. The instrument is so contrived that the height of the barometric column can be ascertained to the tenth of a millimetre, or the four thousandth of an English inch. It is screwed in such a manner in its upper part, as constantly to preserve its verticality. A centigrade thermometer is enchased in its frame.

The hygrometer of Saussure is 2,5 decimetres long, and the arc is divided into 100°. The extreme point of dryness is reduced by calculation, to zero of the scale. A centigrade thermometer constructed by Fortin, is suspended by the side of this instrument, and they are both enclosed in a small case, which preserves them from the dust, the two opposite sides of which are covered with a gauze sufficiently fine to leave them in perfect contact with the atmosphere : These instruments are exposed to the west : they are suspended within 2,4 metres of the earth.

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* Written in French and originally published in the Transactions of the American Philosophical Society of Philadelphia, vol. ii. N. S.

In observations purely thermometric, M. Wallenstein uses two thermometers, graduated on the tubes themselves ; (one constructed by M. Lerebours, an artist attached to the Bureau des Longitudes de Paris, the other by M. Fortin ;) and a thermometer of Mr Troughton's, with the centigrade scale and Fahrenheit's.

As most of M. Wallenstein's instruments are divided according to the metrical scale, he has reduced each observation to the English measure, in order to render his observations more easily comparable with those which have been made in other parts of the country.

He follows the rules of M. Biot respecting the correction for capillary attraction, and for the influence of temperature on his barometric observations. The interior diameter of the tube of his barometer is 9,5 millimetres. He applies a correction of 4,893 millimetres to each of his observations, according to the table of depression calculated by La Place. When this correction is added, the observations are the same as if they had been made with a tube, so large that the mercury could not sink below its true level, as is the case in capillary tubes.

To apply a correction for the temperature of the mercury in the tube of the barometer, he employs the formula given by M. Biot, $(P) = P - \frac{PT}{5412}$, in which (P) represents the corrected height of the barometric column, P the observed height, T the temperature indicated by the thermometer attached to the barometer ; and the constant quantity $\frac{1}{5412}$ represents the true ditation of the mercury for each degree, between the two limits of melting ice and boiling water. The value obtained by applying this correction shows the atmospheric pressure at the temperaature of 32° Fahrenheit.

According to M. Ramond, the most suitable hour for barometrical observation is noon, " because the equilibrium of the atmosphere, altered by the morning winds, is ordinarily restored towards the middle of the day." It is not so easy to determine the other hours ; M. Wallenstein has concluded, however, to make four meteorological observations a day, viz. at 7, 9, noon, and 4 o'clock.

" Among the changes," says M. Wallenstein, " which the atmosphere experiences, that which is indicated by the *horary variations* of the barometer, is not the least curious ; nor has it been well illustrated, even to the present time, either by theory or by observations. M. de Humboldt has, perhaps, considered this subject the most fully in his *Geographie des Plantes.* He there collects the remarks of Godin and La Condamine on the horary oscillations of the barometer under the equator. Godin, according to him, first pointed out this phenomenon. According to La Condamine, these variations take place under the equator at 9 in the forenoon and 3 in the afternoon ; but he does not say at which of these times the barometer is at its maximum or minimum. M. de Humboldt concludes from his own observations, that along the shore of the South Sea, in the plain of the river Amazons, and in places of an elevation of 4000 metres, the times of these variations are constant ; that they are independent of the changes of temperature and of the seasons, and that the barometer is at its maximum at 9, A. M. that it does not vary its height, but slightly, till 12 ; but after noon it falls rapidly till 4 or 4½, when it is at its minimum ; from this time it rises till 11 at night, when it is very nearly as high as it was at 9 the forenoon preceding ; it then falls anew till 4½ o'clock in the morning, when it is almost as low as it was the preceding afternoon at 4 ; finally it rises again till it reaches its maximum at 9.

"In another work, entitled *Experiences sur l'Initation des Fibres Musculaires et Nerveurses,* which is only in part translated into French, and of which the publication preceded by many years that which has just been cited, M. de Humboldt has considered atmospheric variations in their relation to physiology and pathology. 'If there constantly exist *the same difference* between the barometric pressure of two places, if for example in one of these places it was 28 inches, and in the other 24, organized bodies would probably be affected by the unequal density of the air, after the same manner, according to the law of habitual irritation. The rise and fall of the barometer *in the same place,* has a different effect. I have no doubt but that the countries where the variations of the barometer are the least considerable, are also the least healthful.' The salubrity of Peru, Chili, and mountainous regions generally, are cited in support of this opinion. 'I am inclined to believe,' says the same author, in another part of the same work, 'that the nature or the composition of the atmosphere, is less variable under the tropics than in the temperate regions of the globe. All meteorological phenomena seem there to follow a certain law, from which they very rarely vary. The diurnal flux and reflux of heat, of light, of electricity, and of the magnetic fluid are there more equal in their march, at least, so far as the few observations yet made, allow us to judge. Even the barometer, the variations of which, so complicated in our climate, embarrass naturalists so much, rises and falls regularly, under the tropics, four times every twenty-four hours. Ought we to attribute to this regularity in the constitution of the atmosphere, the obstinate character of epidemic diseases in the climate of palms? In this climate when a disease fixes on the vital functions, it makes progress without being arrested by any external cause. The changes of the surrounding fluids are not sufficiently great to check decomposition. It is only at the arrival of a new season, that the power of external exciting causes triumphs over the morbid developement that has taken place in the fibre. In temperate climates, on the contrary, variations in the principle constituants of the atmosphere, are so frequent and so considerable, that they act powerfully on the vital functions. A change in the elasticity of the air, in the electric charge, in the quantity of oxygen, and in the humidity, modifies the vital functions, and the progress of the epidemic is, in part, checked by this continual modification of the external elements, or the disease shortly takes another form. Observing physicians have remarked, that in the temperate zone, places where the temperature is the most variable are the least exposed to epidemic disorders.'

"In this work, published in 1797, M. de Humboldt states the observations of Mr Moseley relative to the horary oscillations of the barometer under the tropics, as well as those of Mr Balfour made in Bengal. According to the *Tableau Physique des Regions Equinoxiales*, printed in 1805, these observations do not agree with those M. de Humboldt has himself made under the tropics.

"In Germany, notwithstanding the numerous perturbations which derange the march of the barometer, we find a certain regularity in the fall of the column of mercury, which takes place at two o'clock P. M., and in its elevation, which takes place at 8 o'clock in the evening.

"M. Cotte has deduced from a great number of observations made in Europe, that the barometer is at its minimum of elevation two hours after noon, and, consequently two hours sooner than under the equator. In our temperate climates the horary variations of the weight of the air, are perhaps concealed under a multitude of local causes, which make the barometer rise and fall irregularly. But with M. Van Swinden, I am far from doubting that the mean terms deduced from many thousand observations made from hour to hour, prove the barometer even in our climate, to rise and fall at certain regular intervals.

" M. Biot states the results to which a long series of observations conducted the celebrated naturalist of the Pyrenees, as regards the subject we are considering. M. Ramond has discovered that in France the barometer attains its maximum elevation about 9 o'clock A. M. after which it descends till about 4 o'clock P. M., when it is at its minimum ; from this time it rises till near 11 P. M., when it reaches its maximum again ; after which it commences a downward motion till 4 A. M., and thence it begins to return to the state first mentioned.

" The astronomer embarked on board the Russian vessels that in 1820 penetrated into high southern latitudes, observed during the navigation between the tropics, ' that the barometer experienced regularly, periodical and daily changes in such a manner that it attained its greatest elevation at 9 in the morning and 9 at night, and fell to the lowest point at 3 in the morning and 3 in the afternoon.'

" I have been for a long time interested in the horary oscillations of the barometer ; indeed I think this subject merits the more attention, when we consider how little it has yet attracted the notice of scientific travellers. The same is the case with the diurnal variations of the magnetic needle, which few naturalists since Van Swinden have studied with attention and perseverance equal to his. Perhaps good observations on the periodical variations of the magnetic needle, of electricity, and of the barometer, collected by such men as La Condamine, Mutis, Humboldt, and Buch, may lead to the discovery of some connexion between these phenomena, or of the means of explaining them one by another.

" At least we have no reason to consider the time we occupy with these observations thrown away, while we know that the subject has engaged the attention of the philosophers we have just named, that a connexion has been remarked by one of them (M. Mutis) between the horary variations of the barometric column, and the conjunctions and oppositions of the moon, and that La Place has undertaken to calculate the influence that the sun and moon may have upon the atmosphere. We have need however, of all the authority of these great names to assist us in bearing up against the disfavour, and almost the ridicule which has been cast on this part of natural science by an astronomer, (M. Bode) renowned for his long and useful labours."

Meteorological Observations for November 1825, *made at Washington, D. C. Lat.* 38° 58′ *Long.* 77° 2′ *by J. De Wallenstein. Esq.*

		Thermom.			Barometer.							
Date.	Hours.	Fahrenheit.	Average.	Hygrometer.	Apparent.	Therm. Barom.	Correction for the Temperature.	Corrected.	Inches.	Average.	Observations and Appearance of the Heavens.	Winds.
		°		°	′	cent						
	7	39.2		92.0	0.7512	12.0	0.00168	0.7500	29.528		clear and calm.	NW.
1	12	61.7	55.1	68.0	0.7510	18.0	0.00252	0.7490	29.489	29.461	light wind.	South
	4	64.4		63.0	0.7480	18.5	0.00259	0.7469	24.367		cloudy, id.	South
	8	45.5		82.9	0.7552	13.0	0.00182	0.7539	29 682		fine weather, calm.	South
2	12	57.0	50.2	,,	0.7567	16.0	0.00224	0.7550	29.725	29.727	clear strong wind.	West
	4	58.0		,,	0.7582	17.0	0.00238	0.7563	29.776		id. id.	id.
	8	36.5		85.0	0.7662	9.0	0.00126	0.7655	30.119		fine, calm.	NNW.
3	12	53.6	48.8	70.0	0.7678	13.0	0.00182	0.7665	30.178	30.141	id. id.	South
	4	56.3		64.0	0.7672	18.0	0.00255	0.7652	30 127		id. id.	id.
	8	36.3		89.0	0.7697	10.0	0.00160	0.7686	30.261		fine, calm.	SW. b. S.
4	12	56.8	49.9	75.0	0.7705	13.0	0.00182	0.7692	30.294	30.277	id. id.	South
	4	56.6		71.0	0.7701	16.0	0.00224	0.7690	30.276		id. id.	South
	8	39.2		92.0	0.7719	11.0	0.00154	0 7709	30.351		hazy, calm.	NNW.
5	12	61.3	54.6	88.0	0.7720	16 0	0.00224	0.7703	30.328	30.319	clear, id.	South
	4	63.5		75.0	0.7711	18.0	0.00252	0.7691	30.280		id. id.	SSE.

	8	43.3		84.0	0.7697	13.0	—	0.00182	0.7684	30.253		light fog, calm.	SSE.
6	12	71.6	61.8	70.0	0.7687	19.0		0.00266	0.7666	30.182	30.188	clear, id.	S. b. W.
	4	70.5		68.0	0.7675	19.6		0.00274	0.7653	30.131		id. id.	South
	8	50.7		90.0	0.7659	15.5		0.00217	0.7652	30.127		light fog.	NW.b.N.
7	12	68.0	59.2	66.0	0.7660	19.0		0.00266	0.7629	30.036	30.053	fine, calm.	South E.
	4	59.0		60.0	0.7640	19.0		0.00266	0.7619	29.996		id. id.	N. b. E.
	8	42.4		86.0	0.7619	12.0		0.00168	0.7607	29.950		light fog, calm.	NW. b.N.
8	12	66.2	57.5	64.0	0.7602	12.5		0.00175	0.7590	29.882	29.874	clear, calm.	South
	4	63.9		65.0	0.7587	18.0		0.00252	0.7667	29.792		cloudy.	South
	8	46.9		,,	0.7595	13.0		0.00182	0.7582	29.851		overcast, strong wind.	N. b. W.
9	12	42·6	45.6	100.0	0.7590	15.5		0.00217	0.7573	29.816	29.834	rain, id.	North
	4	47.3		,,	0.7592	14.0		0.00196	0.7578	29.835		cloudy, id.	North
	8	39.2		90.0	0.7609	11.0		0.00154	0.7599	29.917		fine weather, calm,	NNW.
10	12	50.4	46.5	66.0	0.7610	11.7		0.00164	0.7600	29.922	29.915	id. id.	W. b. N.
	4	50.0		64.0	0.7610	14.0		0.00196	0.7586	29.906		id. id.	N. b. W.
	8	34.2		98.0	0.7597	8.0		0.00112	0.7591	29.881		cloudy, calm	SW. b.W.
11	12	53.2	47.0	74.0	0.7590	13.0		0.00182	0.7577	29.832	29.850	id. id.	South
	4	53.6		,,	0.7595	14.0		0.00196	0.7580	29.843		id. id.	North
	8	40.6		70.0	0.7656	4.0		0.00056	0.7655	30.139		fair, calm.	West
12	12	49.1	47.1	54.0	0.7657	15.0		0.00210	0.7641	30.083	30.103	id. light wind.	W. b. N.
	4	51.8		48.0	0.7657	14.0		0.00196	0.7142	30.087		id. calm.	North
	8	45.1		76.0	0.7657	9.0		0.00126	0.7650	30.119		fair, calm	South
13	12	55.8	53.1	60.0	0.7632	12.5		0.00175	0·7620	30.001	30.015	id. brisk wind.	South
	1	55.4		65.0	0.7616	14.0		0.00196	0.7601	29.926		id. id.	SSE.
	8	55.0		100.0	0.7547	16.0		0.00224	0.7530	29.646		rain, strong wind.	North
14	12	52.7	52.5	,,	0.7567	16.0		0.00224	0.7550	29.725	29.711	cloudy, wind abating.	NW.
	4	50.0		,,	0.7580	15.0		0.00210	0.8567	29.480		fair. calm.	"
	8	42.2		95.0	0.7602	11.6		0.00164	0.7592	29.890		cloudy, light wind.	South
15	12	55.0	49.8	60.0	0.7605	14.5		0.00203	0.7590	29.882	29.883	id. id.	id.
	4	52.3		75.0	0.7605	15.0		0.00210	0.7589	29.878		id. id.	SE. b. S.

Date.	Hours.	Thermom.		Hygrometer.	Barometer.						Observations and Appearance of the Heavens.	Winds.
		Fahrenheit.	Average.		Apparent.	Thermom. Barom.	Correction for the Temperature.	Corrected.	Inches.	Average.		
		°		°	'	cent						
	8	42.3		80.0	0.7597	9.8	— 0.00137	0.7588	29.874		clear, calm.	North
16	12	46.8	44.5	68.0	0.7600	12.8	0.00179	0.7587	29.870	29.875	id. light wind.	West
	4	44,6		72.0	0.7600	11.0	0.00154	0.7590	29 882		id. id.	NW.
	8	40.8		90 0	0.7559	11.0	0.00154	0.7549	29.721		cloudy calm.	South
17	12	46.2	43.3	92.0	0.7522	13.0	0.00182	0.7509	29.564	29.602	id. id.	S. b. W.
	4	42.8		92.0	0.7509	12.0	0.00168	0.7497	29.517		id. id.	"
	8	30.5		,,	0.7557	7.7	0 00098	0.7552	29.733		clear, violent wind.	NW.
18	12	,,	31 2	,,	0.7565	6.8	0.00095	0.7561	29.768	29.788	,, ,,	NW.
	4	32.0		,,	0.7590	7.0	0.00098	0.7585	29 863		,, ,,	NW.
	9	30.2		68.0	0.7650	3.5	0.00049	0.7656	30.119		fair, calm.	NNE.
19	12	38.7	35.4	60.0	0.7630	8.0	0.00112	0.7644	30.095	30.083	id. id..	SW b.W.
	4	37.4		65.0	0.7636	8.5	0.00119	0.7629	30.036		id. id.	South.
	8	39.2		75.0	0.7605	6.5	0.00091	0.7601	29.926		cloudy, light wind.	S. b. E.
20	1	51.8	47.0	57.0	0.7600	10.0	0.00140	0.7591	29.886	29,895	fair, calm.	W. b. N.
	4	50.0		62.0	0.7600	12.0	0.00168	0.7588	29.874		id. id.	SSE.
	8	37 0		97.0	0.7629	7.0	0 00098	0.7624	30.017		hazy, calm.	North
21	12	53.6	45.2	65.0	0.7631	11.5	0.00161	0.7620	30.001	29.997	cloudy, id.	SE.
	4	45.1		88.0	0.7625	12.0	0.00168	0.7613	29.773		id. id.	South
	8	35.4		94.0	0.7624	9.0	0.00126	0.7617	29.989		foggy, calm.	NW.
22	12	49.3	45.8	79.0	0.7625	10.6	0.00148	0.7615	29.981	29.990	id. brisk wind.	NNW.
	4	52.7		00.0	0.7635	14.7	0.00205	0.7629	30.001		light fog, calm.	NW.

	8	25.3		98.0	0.7674	4.8	— 0.00067	0.7672	30.205		fair, calm.	NNW.
23	12	11.0	37.2	81.0	0.7667	10 0	0.00140	0.7658	30.150	30.137	id. id.	South
	4	45.5		78.0	0.7645	10.0	0.00140	0.7636	30.064		id. id.	S. b. E.
	9	39.2		100.0	0.7600	8.5	0.00119	0.7597	29.908		cloudy, calm.	SW.
24	12	46 4	45 6	85 0	0.7596	11.0	0.00154	0.7586	29.867	29.878	id. id.	NW.
	4	51.3		64.0	0.7596	12.0	0.00168	0.7584	29.859		id. brisk wind.	NW.
	9	41.0		94.0	0.7610	8.0	0.00126	0.7603	29.934		fair and calm.	NW.
25	"	"	44.1	"	"	"	"	"	"	29.916	"	"
	4	47 3		69 0	0.7610	15.0	0.00210	0 7594	29.898		id. id.	id.
	9	32.9		99.0	0.7628	7 6	0.00106	0·7623	30 013		fair, calm,	NW.
26	12	48.2	43.2	75.0	0.7630	10.0	0.00140	0.7621	30 005	30.003	id. id.	id.
	4	48.7		73.0	0.7630	12.0	0.00168	0.7618	29.992		id. id.	NE.
	8	25.0		98.0	0.7633	5.8	0.00081	0.7630	30.040		fair, calm, white frost.	NNW.
27	12	49 0	41.3	85.0	0 7632	9.5	0.00133	0.7624	30.017	30.007	fair, calm.	NNW.
	4	50.0		80.0	0.7630	17.5	0.00243	0.7611	29.965		id. id.	East
	8	31.0		92.0	0.7634	10.0	0.00140	0.7625	30.021		fair, calm,	NW.
28	12	47.0	41.6	80.0	0.7632	12.5	0.00175	0.7620	30 001	30.010	id. id.	NW.
	4	47.0		71.0	0.7632	10 5	0.00147	0.7622	30.009		id. id.	East
	8	26.0		90.0	0.7630	17.5	0.00243	0.7611	29.965		fair, calm.	NE.
29	12	49.0	41.8	76.0	0.7632	11.5	0 00161	0.7621	30.005	29.970	id. id.	SE.
	4	50 5		74.	0.7626	18.0	0.00252	0.7606	29.946		id id.	SE.
	8	40 0		93.0	0.7611	9.0	0.00126	0.7603	29.934		cloudy, calm.	NE.
30	12	43.5	42.2	92.0	0.7610	13.5	0.00189	0.7596	29.906	29.901	id. id.	North
	4	43 0		94.0	0.7603	14.0	0.00196	0.7588	29.874		id. id.	W. b. N.

THERMOMETER.

Average Heat, 48°,2
Maximum the 6th at 12 o'clock, 71, 6
Minimum the 27th, at 9 A. M. 25, 0

Range, 46,6

BAROMETER. Inches.

Average Height, 29,944
Maximum the 5th, at 12 o'clock, 30,328
Minimum the 1st at 4 P. M. 29,367

Range, 0,961

HYGROMETER.

Average dampness of the air, 76°,7
Greatest do. (rain), 100, 0
(The 12th at 12,) least dampn. 64, 0

Difference, 46, 0

Number of rainy days, 2
Total rain, 0,24 inch.

Abstract of Meteorological Observations for the year 1825, *taken in Boston, in Lat.* 42° 21′ 58″ *N., Long.* 71° 3′ 45″ *W. Communicated by R. T. Paine, Esq.*

	Barometer.		Thermometer.	
January.				
Mean altitude,		30,006		29,1
Highest,	22d,	30,56	12th,	49
Lowest,	2d	28,99	8th,	8
Range,		1,57		41
February.				
Mean altitude,		30,023		32,0
Highest,	8th,	30,58	24th,	51
Lowest,	2d,	29,30	4th,	7
Range,		1,28		44
March.				
Mean altitude,		29,918		40,8
Highest,	16th,	30,38	20th,	65
Lowest,	31st,	29,34	23d,	28
Range,		1,04		37
April.				
Mean altitude,		29,960		50,1
Highest,	2d,	30,44	16th,	80,
Lowest	11th,	29,48	2d,	33
Range		,96		47
May.				
Mean altitude,		30,025		57,6
Highest,	15th,	30,36	31st,	81
Lowest,	5th,	29,58	3d,	34
Range,		,78		47
June.				
Mean altitude,		29,884		70,1
Highest,	30th,	30,11	11th,	96
Lowest,	25th,	29,42	4th.	53
Range,		,69		43
July.				
Mean altitude,		29,939		76,4
Highest,	9th,	30,17	11th,	100
Lowest,	24th,	29,74	28th,	60
Range,		,43		40
August.				
Mean altitude,		29,978		69,5
Highest,	19th,	30,32	6th,	89
Lowest,	9th,	29,62	27th,	54
Range,		,70		35
September.				
Mean altitude,		29,993		61,1
Highest,	12th,	30,30	15th,	79
Lowest,	24th,	29,46	25th,	45
Range,		,84		34

OCTOBER.

Mean altitude,		30,004		54,4
Highest,	22d,	30,49	7th,	88
Lowest,	12th,	29,56	22d,	30
Range,		,93		58

NOVEMBER.

Mean altitude.		30,147		41,6
Highest,	4th,	30,70	6th,	65
Lowest,	18th,	29,46	19th,	17
Range,		1,24		48

DECEMBER.

Mean altitude,		29,965		33,2
Highest,	23d,	30,58	17th,	58
Lowest,	31st,	29,41	13th,	—5
Range,		1,17		63

Mean of 1st 6 months,		29,969		46,70
„ „ last 6 do.		30,004		56,10
„ of the year,		29,987		51,44
Highest.	Nov 4th,	30,70	July 11th,	100
Lowest,	Jan 2d,	28,99	Dec. 13th,	—5
Range during the year,		1,71		105

The mean temperatures of all the months in the last year, except August and September, are from two to five degrees greater than those deduced from observations of the last thirty years; and the mean of the whole year is 2,84 degrees greater.

Three observations were made in each day, at sunrise, 3 P. M., in summer, 2½ P. M when the sun was on the equator, and 2 P. M. in the winter, and at 10 in the evening. The mercury in the cistern of the barometer is fourteen feet above the mean level of the sea.

General Intelligence.

On the Thermometrical State of the Terrestrial Globe.—M. Arago, in an article in the "Annales de Physique," discusses the question of the temperature of the globe at its surface and arrives at this conclusion, that in Europe in general, and in particular in France, the winters some centuries back, have been as cold as at present. He grounds his opinion upon the fact of the freezing of the rivers and seas at a great number of periods even of very remote date. The author then gives a table of the extreme temperatures observed at Paris, from which there results that in the second half of the last century, the greatest cold (—23,5° cent.) took place in the

25th January, 1795, and the greatest heat (38,4°) on the 8th July, 1793. He then gives the temperatures observed during the expeditions of Captains Parry and Franklin, and the dates of the natural congelation of mercury, together with the tables of the maximum temperatures of the atmosphere observed on the open sea at its surface. From these observations together, M. Arago draws the following conclusions: 1st, In no part of the earth on land, and in no season, will a thermometer, raised from 2 to 3 metres above the ground and protected from all reverberation, attain the 46 centigrade degree; 2dly, In the open sea, the temperature of the air, whatever be the place or season, never attains the 31st centigrade degree; 3dly, The greatest degree of cold which has ever been observed on our globe with a thermometer suspended in the air, is 50 centigrade degrees below zero; 4thly, The temperature of the water of the sea in no latitude, and in no season, rises above 30 centigrade degrees. [*Ann. de Phys. et de Chim.*]

Hawkins's Mode of preparing Emery.—Mr Hawkins, finding that the emery sold in the shops was totally inefficient for the purpose he had in view, namely, grinding two flat surfaces of hard cast steel accurately, thought of applying a process he had seen for washing over *diamond dust*, to emery; and to be certain that his emery was of good quality, he purchased of an emery-maker a quantity of those small lumps, or grains, which had longest withstood the action of the cast-iron runners and bed, and thus ensured the hardness of the emery; these pieces were reduced to powder in a cast-iron mortar, and then separated into different portions by sieves.

He then washed over the finest emery thus obtained, using oil instead of water, as in the usual process, the former holding it in suspension for a much longer time; and in this way obtained a series of emery which had floated one, five, ten, fifteen, twenty, forty, and eighty minutes, amongst which he fonnd every variety necessary for his purpose; and keeping them in boxes, which were numbered according to the minutes they had floated, he could at any time prepare more of any one kind. In this way Mr Hawkins readily attained his object, and ultimately by selecting those grains of emery which resisted longest the action of the pestle and mortar, he obtained some so hard, as to be capable of cutting a ruby, when employed instead of diamond-dust.

Mr Gill by grinding Greek emery-stone between two flat and hard steel surfaces, and washing off the lighter portions in oil, found that those which subsided in half a minute, when examined by a microscope, had entirely resisted the friction, and were perfectly crystallized sapphires. [*Tech. Rep.*]

On the Umbilicus of Marsupial Animals.—It has been generally thought that Marsupial animals are destitute of any umbilicus. and are only attached to the mother by means of the mouth.

Geoffroy St Hilaire has lately discovered in some specimens of the fœti of *Didelphis Virginiana* preserved in spirits, which had been taken from the mother by Dr Barton very soon after their introduction into the pouch, evident vestiges of placental organization, and of an umbilicus. They were only five lines long, and already formed ; in the two males which he examined, the umbilicus was large for the size of the animal, as it was also in the female, and very distinct from the entrance of the pouch.

Mr Geoffroy observes, that the series of transformations common to all mammalia are *ovulum*, *embryo*, and fœtus. These three stages of genital products require three distinct situations, which in the other mammalia are found in the sexual canal, but in the marsupialia they are very differently distributed, though in an equally continuous series. The ovulum and the embryo are formed and developed in the sexual canal and the fœtus out of it. The womb is the third station in the mammalia, where the common fœtus is incubated and nourished ; and the *marsupium* or nursing pouch is for the same purpose in the marsupial animals. The difference, therefore, consists only in the name of the last part. [*Ann. Sci. Nat. and Zool. Jour.* 1. 403.]

Capillary Attraction.—M. Gillerson says, " If a capillary tube be introduced into mercury, the metal will remain in the tube below the exterior surface. If then the tube be carefully raised, without taking it out of the mercury, the surface of the mercury in the tube may be raised to the level of that without. Operating very carefully, it may even be raised still higher ; its surface will then become concave, the nature of the curve apparently approaching that of the catenarian curve, which I believe also to be that of liquids which in capillary tubes are raised above the level of the external surface. If then the tube be depressed a little, the convex surface may be again given to the mercury in the tube, without its level being depressed below that of the external portion. It appears to me therefore, that the surface of liquids in capillary tubes is an accessary circumstance, and has no direct influence on the elevation or depression of the liquid." [*Bib. Univ.* XXII. 209.]

On Combinations of Carbon and Iron, Pig Iron, &c.—A long experimental memoir by M. Karsten, on the combination of iron and carbon, is contained in the Annales des Mines, IX. 657. We have not time at present to analyze it, but give the results as drawn up by the author, and appended to the memoir.

1. White cast iron and tempered steel contains the carbon combined with the whole mass of iron. 2. Lamellated white cast iron presents a perfect combination of iron with carbon; it always contains more carbon than the grey cast iron. 3. Iron and steel, not tempered, contains the carbon in the state of carburet. 4. Cold grey cast iron contains the larger part of its carbon, in the state of graphite and of mixture: This graphite contains no iron, but constitutes the carbon in all its purity. 5. The rest of the carbon contained in the grey cast iron, may be found either combined with the whole mass, or forming a definite carburet which is afterwards dissolved in the metal, as is the case with soft iron, or steel. 6. All the varieties of carburetted iron, considered in the liquid state, contain the carbon dissolved in the mass of metal in indefinite proportions. 7. Finally. The graphite separates from the metal at the moment of congelation, and if there be other carburets of iron, they separate at a later period.

Remarking on the means generally proposed for the separation or estimation of the carbon in carburetted iron, M. Karsten finds grounds of objection to them all. That proposed by M. Vauquelin, namely, the use of the sulphurous acid, he states to be uncertain and inacurate from the formation of sulphuret of iron. [*Jour. of Sci. Lit. and Arts.*]

Antiquity of Trees.—In "Major Rooke's Sketch of the Forest of Sherwood," it is stated that in cutting down some timber in Berkland and Bilhaugh, *letters* have been found cut or stamped in the body of the trees, denoting the king's reign in which they were marked. It seems that the bark was cut off, and the letters cut in, after which the next year's wood grew over it, but without adhering where the bark had been cut. The ciphers are of James I., of William and Mary, and one of King John! One of those with James's cipher was about one foot within the tree, and one foot from the centre: it was cut down in 1786. The tree must have been two feet in diameter, or two yards in circumference, when the mark was cut. A tree of this size is generally estimated at 120 years growth, which number substracted from the middle year of James's reign would make 1492 the date of the planting of the tree. The tree with William and Mary had the mark about nine inches within the tree, and three feet three inches from the centre; cut down also in 1786. The mark of John was eighteen inches within the tree, and something more than a foot from the centre: it was cut down in 1791; but the middle year of John's reign was 1207, from which if we substract 120, the number of years requisite for a tree two feet diameter to arrive at that growth, it will make the date of its planting 1085, or about twenty years after the conquest. The tree therefore, when cut down in 1791, must have been 706 years old; a fact scarcely credible; for it appears from the trees whose marks are better authenticated, that those exactly of the same size, when marked, had increased twelve inches in diameter in 173 years, whilst this tree had increased no more than eighteen inches in 584 years. Major Rooke says that several trees with this mark had been cut down, so that deception or mistake is scarcely possible. [*N. M. Mag.*]

Light of Haloes.—M. Arago, from observations made on the 11th April, 1825, with the instrument which he has invented for the examination of polarized light, has discovered that the light of haloes (luminous circles which sometimes appear round the sun, and whose apparent diameters are $22\frac{1}{2}^{\circ}$ and 45°) is not a reflected, but a refracted light; a result which gives much probability to the explanation of the phenomenon proposed by Mariotte. This philosopher supposed that the solar ray is refracted in its passage through the drops of water frozen and suspended in the atmosphere. M. Arago is of opinion, that the observation of haloes might lead to the discovery of the true law of the decrease of temperature in proportion as we rise from the earth's surface, a law which hitherto has had no other foundation than a single ærostatic ascension of Gay-Lussac.—[*Bullet. Univ. May*, 1825.]

Sir H. Davy's method of protecting the Copper Sheathing of Ships.—Several accounts have been published in this Journal of the labours of Sir H. Davy in attempting to protect the copper sheathing of ships from corrosion. An unusual excitement has prevailed in England, upon this subject, for more than a year past; an excitement which may be attributed more perhaps to the high character of Sir H. Davy, than to the importance of the experiments in which he has been engaged. It appears by late accounts in English publications, that ships protected by iron, have been found so generally *foul*, both with the fleshy and hard shell species of *barnacles* after a voyage, that the admiralty have given orders to suspend the use of the protectors on all national sea going ships; but they are to be continued on vessels in ordinary, receiving ships, hulks, &c.

A correspondent of this Journal has favoured us with a short paper on this subject; part of which, being a recapitulation of preceding accounts, it is not necessary to publish. The following are extracts from this paper:—

"If I am right in the conjecture, the copper sheathing, as generally used, does not become foul, because the rust that readily wastes it in the sea, is so peculiarly corrosive and destructive, that nothing possessing animal or vegetable life will adhere to it. The difficulty is, that the new method, while it saves the copper, prevents the oxidation, which keeps it clear of these destructions, and unless some remedy shall be discovered, the method must undoubtedly be given up.

"This idea has occurred to me, that the plates of iron, zinc or tin, may be secured to the vessel in such a manner, as to be capable of being occasionally removed at sea, so as to allow a partial oxidation of the copper sufficient to detach any of the vegetable or animal substances that may be collecting, and then replaced. They might no doubt be fixed so as to be connected with the copper, and be continually wet, which would give them the operation desired, and still be within reach. Sir H. Davy found that a small mass of tin, even when communicating only by a wire with a large plate of copper, entirely preserved it. Experiments might easily be made to ascertain whether weeds and barnacles would not fall off from copper, when oxidation was suffered to take place; and also for how long a time it would be necessary the metal should be subjected to the action of sea water, in order to effect such oxidation to a sufficient extent."

If no experiments founded on views like these, have been made, we hope that they will be instituted upon these suggestions, as it would certainly appear that a considerable reduction of the oxidation, by means of the protectors, is not incompatible with a clean state of the copper.

ART. XXIV.—*Biographical Account of M. le Comte Lagrange.* By M. le Chevalier Delambre. [*Ann. Philos.*]

(Concluded from p. 114.)

M. LAGRANGE took possession of his situation on the 6th of November, 1766. He was well received by the king; but soon perceived that the Germans do not like to see foreigners occupy situations in their country. He applied to the study of their language. He devoted himself entirely to mathematics, and did not find himself in the way of any person, because he demanded nothing; and he soon obliged the Germans to give him their esteem. "The king," said he himself, "treated me well, I thought that he preferred me to Euler, who was something of a devotee, while I took no part in the disputes about worship; and did not contradict the opinion of any one." This prudent reserve, if it deprived him of the advantage of an honourable familiarity, which would have been attended with some inconveniences, left him the whole of his time for mathematical labours, which hitherto had brought him nothing but compliments the most flattering and the most unanimous. This concert of praises was only once interrupted during the whole of his life.

A French mathematician, who to much sagacity united a still greater degree of selfishness, and scarcely gave himself the trouble to study the works of others, accused M. Lagrange of *having gone astray in the new route that he had traced, from not having well understood the theory of it.* He reproached him with *having deceived himself in his assertions and calculations.* Lagrange in reply expresses some astonishment at these harsh expressions, to which he was so little accustomed. He expected at least to have seen them founded upon some reasons either good or bad; but he discovered nothing of the kind. He shows that the solution proposed by Fontaine was incomplete and illusory in certain respects. Fontaine had boasted that he had taught mathematicians the conditions which render possible the integration of differential equations with three variables. Lagrange showed him by several citations, that these conditions were known to mathematicians long before Fontaine was capable of teaching them. He does not deny that Fontaine discovered these theorems himself, "at least I am persuaded," says he, "that he was as capable of finding them as any person whatever."

It was with this delicacy and moderation that he answered the aggressor. Condorcet, in his *éloge* of Fontaine, is obliged to avow that, on this occasion, his friend deviated from that politeness which ought never to be dispensed with, but which perhaps he thought less necessary with illustrious adversaries, whose glory did not stand in need of these little delicacies. Every one can estimate the value of that apology, especially when applied to a man who, by his own acknowledgment, studied the vanity of others that he might wound it upon occasion. We must at least acknowledge that he, who saw himself attacked in that manner when he was in the right, and who knew how to maintain politeness with an adversary who had himself dispensed with it, acquired a double advantage over him, besides victoriously repelling his imprudent attack.

It will not be expected that we should follow M. Lagrange in the important researches with which he filled the Berlin Memoirs; and even some volumes of the Memoirs of the Turin Academy, which was indebted to him for its existence. All the space that can be devoted to this biographical account would not be sufficient even to give an imperfect idea of the immense series of his labours, which have given so much value to the Memoirs of the Berlin Academy, while it had the inestimable advantage of being directed by M. Lagrange. Some of these Memoirs are of such extent and importance that they might pass for a great separate work, yet they constitute a part only of what these twenty years enabled him to produce. He had composed his Mecanique Analytique, but he wanted to have it printed at Paris, where he expected that his formulas would be given with more care and fidelity. On the other hand, it was running too great a risk to intrust the manuscript into the hands of a traveller, who might not be aware of the whole of its value. M. Lagrange made a copy of it, which M. Duchatelet undertook to deliver to the Abbé Marie, with whom he was intimately connected. Marie fulfilled with honour the confidence placed in him. His first care was to find a bookseller who would undertake to publish it; and, what it will be difficult to believe at this time, he could not find one. The newer the methods in it were, and the more sublime the theory, the fewer readers would be found capable of appreciating it; hence, without entertaining any doubts of the merit of the work, the booksellers were excusable in hesitating to print a book, the sale of which would probably be confined to a small number of mathematicians disseminated through Europe. Desain, who was the most enterprizing of all those to whom application was made, would not undertake to publish it, till Marie entered into a formal engagement to take all the copies of the edition which were not sold by a given time. To this first service Marie added another, of which M. Lagrange was not less sensible; he procured him an editor worthy of superintending the publication of such a work. M. Legendre devoted the whole of his time to the troublesome task of correcting the press, and was repaid by the sentiment of veneration for the author with which he was penetrated; and by the thanks which he received from him in a letter which I have had in my possession, and which M. Lagrange had filled with expressions of his esteem and his gratitude.

The book was not yet published when the author came to settle in Paris. Several causes determined him to take this step; but we must not give credit to all that have been stated. The death of Frederic had occasioned great changes in Prussia, and still greater were to be apprehended. Philosophers were no longer so much respected as formerly. It was natural for M. Lagrange again to feel that desire which had formerly conducted him to Paris. These causes, together with the publication of the Mecanique Analytique, were sufficient. It is not necessary to add other causes, which several publications that made their appearance in Germany, and particularly the anonymous historian of the court of Berlin, have noticed. We never, during a residence of twenty-five years in France, heard M. Lagrange prefer the slightest complaint against the minister, who is accused in that publication of having disgusted him by a treatment full of haughtiness and contempt, which out of respect for himself it was impossible for M. Lagrange to overlook. We might suspect that M. Lagrange had sufficient generosity to forget or pardon bad treatment, which he punished in the only way worthy of himself, by leaving the country where his merit was overlooked; but when he was directly questioned on that subject by a Member of the Institute (M. Burckhartd) he only gave negative answers, and assigned no other motives than the misfortunes which it was thought were about to fall upon Prussia. M. de Hertzberg was dead, and M. le Lagrange, a senator and comte of the French empire, could have no interest in concealing the truth. Hence we must consider his own statement as affording the only true reasons.

The historian therefore, whom we have quoted, has been ill-informed. But the spirit of calumny and satire, which has so justly rendered his work suspected, ought not to prevent us from extracting from it the lines in which he explains, with that energy which is peculiarly his own, his opinion, which is that of all Europe, when he does justice to M. Lagrange.

"I think," says he, (Hist. Secrete de la Cour de Berlin, 1789, tome ii. page 173), "that there is at this moment an acquisition worthy of the king of France, the illustrious Lagrange, the greatest mathematician who has appeared since Newton, and who in every point of view is the man that has the most astonished me; —Lagrange, the wisest, and perhaps the only practical philosopher that ever existed, meritorious by his undisturbable wisdom, his manners, his conduct; the object of the most tender respect of the small number of men with whom he associates;—Lagrange is misunderstood; every thing leads him to leave a country where nothing can excuse the crime of being a foreigner, and where in fact he is merely tolerated. Prince Cardito de Laffredo, Neapolitan minister at Copenhagen, offered him the most flattering conditions on the part of his sovereign. The Grand Duke, the King of Sardinia, invite him eagerly; but all their proposals would be easily obliterated by ours. I am very eager to see this proposal made, because I consider it as noble, and because I tenderly love the man who is the object of it. I have induced M. Lagrange not to accept immediately the proposals made to him, and to wait till he receives ours."

The author whom we quote appears to fear the opposition of M. Breteuil; but, according to M. Lagrange himself, it was the Abbé Marie who proposed it to M. Breteuil, who on all occasions anticipated the desires of the Academy of Sciences, presented the demand to Louis XVI, and induced him to agree to it.

The successor of Frederic, although he did not much interest himself in the sciences, made some difficulty in allowing a philosopher to depart whom his predecessor had invited, and whom he honoured with his particular esteem. After some delay, M. Lagrange obtained liberty to depart. It was stipulated that he should still give some memoirs to the Berlin Academy. The volumes of 1792, 1793, and 1803, show that he faithfully kept his promise.

It was in 1787 that M. Lagrange came to Paris to take his seat in the Academy of Sciences, of which he had been a foreign member for fifteen years. To give him the right of voting in all their deliberations, this title was changed into that of *veteran pensionary.* His new associates showed themselves happy and proud in possessing him. The Queen treated him with regard, and considered him as a German. He had been recommended to her from Vienna. He obtained a lodging in the Louvre, where he lived happy till the Revolution.

The satisfaction which he enjoyed did not show itself outwardly. Always affable and kind when interrogated, he himself spoke but little, and appeared absent and melancholy. Often in companies which must have been suitable to his taste, among the most distinguished men of all countries who met at the house of the illustrious Lavoisier, I have seen him dreaming as it were, with his head against a window, where however nothing attracted his attention. He remained a stranger to what was passing around him. He acknowledged himself that his enthusiasm was gone, that he had lost his taste for mathematics. When informed that a mathematician was employed at such a task, "so much the better," he would say, "I had begun it, now it will be unnecessary for me to finish it." But he merely changed the object of his studies. Metaphysics, the history of human nature, that of different religions, the general theory of languages, medicine, botany, divided his leisure hours. When the conversation turned upon subjects with which it was supposed he was unacquainted, we were struck by an unexpected observation, a fine thought, a profound view, which excited long reflections. Surrounded by chemists who were reforming the theory and even the language of the science, he made himself acquainted with their discoveries, which gave to facts formerly isolated, that connexion which distinguishes the different parts of the mathematics. He undertook to make himself acquainted with this branch of knowledge, which formerly appeared to him so obscure, but which he found on trial *as easy as algebra*. People have been surprised at this comparison, and have thought that it could come from no one else than Lagrange. It appears to us as simple as just; but it must be taken in its true sense. Algebra, which presents so many insoluble problems, so many difficulties against which Lagrange himself struggled in vain, could not in that sense appear to him an easy study. But he compares the new elements of chemistry with those of algebra. They constituted a body, they were intelligible, they offered more certainty, they resembled algebra, which in the part of it which is complete presents nothing difficult to conceive, no truth to which we may not arrive by the most palpable reasoning. The commencement of the science of chemistry appeared to him to offer the same advantages, perhaps with somewhat less stability and certainty; but, like algebra, it has no doubt also its difficulties, its paradoxes, which will require, to explain them, much sagacity, reflection, and time. It has likewise its problems, which never will be resolved.

In this philosophic repose he continued till the revolution, without adding any thing to his mathematical discoveries, or even opening his Mecanique Analytique, which had been published for two years.

The revolution gave philosophers an opportunity of making a great and difficult innovation: the establishment of a system of weights and measures founded on nature, and perfectly analogous to our scale of numbers. Lagrange was one of the commissioners whom the Academy charged with that task. He was one of its keenest promoters. He wished to see the decimal system in all its purity. He was provoked at the complaisance of Borda, who got quarters of the *metre* made. He thought the objection of little importance which was drawn against the system from the small number of divisors that its base afforded. He regretted that it was not a prime number, as 11, which would have given the same denominator to all the fractions. This idea perhaps will be regarded as one of those exaggerations, which are hazarded by men of the best understandings, in the heat of dispute. But he mentioned the number 11 merely to get rid of the number 12, which more intrepid innovators would have wished to substitute in place of the number 10, that constitutes the base of the whole of our numeration.

When the Academy was suppressed, the commission charged with the establishment of the new system was retained for a time. Three months had scarcely elapsed when, in order to *purify* that commission, the names of Lavoisier, Borda, Laplace, Coulomb, Brisson, and Delambre, were struck out. Lagrange was retained. In quality of president, he informed me, in a long letter full of kindness, that I should receive official information of my removal. As soon as he saw me on my return to Paris, he expressed to me his regret at the dismission of so many associates. "I do not know," said he, "why they have retained me." But unless the suppression had been total, it could scarcely have extended to him. The more losses the commission had sustained, of the more importance was it not to deprive it of the consideration attached to the name of Lagrange. Besides, he was known to be wholly devoted to the sciences; he had no place either in the civil department or the administration. The moderation of his character had prevented him from expressing what he could not but think in secret: but I shall never forget the conversation which I had with him at that period. It was the day after the atrocious and absurd sentence, contrary to every thing like justice, had thrown all lovers of the sciences into mourning, by cutting off the most illustrious philosopher in Europe. "It has cost them but a moment," said he, "to cut off that head, and a hundred years perhaps will not be sufficient to produce another like it." Some months before we had had a similar conversation in the cabinet of Lavoisier, on account of the death of the unfortunate Bailly. We lamented together the dreadful consequences of the dangerous experiment which the French had attempted. All these chimerical projects of melioration appeared to him very equivocal proofs of the greatness of the human mind. "If you wish to see it truly great," added he, "enter into the cabinet of Newton employed in decomposing light, or in explaining the system of the world."

Already for some time he had regretted not having listened to the advice of his friends, who at the commencement of our troubles had recommended him to seek an asylum, which it would have been so easy for him to find. As long as the revolution seemed only to threaten the pension which he enjoyed in France, he had neglected that consideration, out of curiosity to be upon the spot of one of those great convulsions which it is always more prudent to observe at a distance. "It was your own choice," said he several times to himself when he entrusted me with his regret. It was to no purpose that a special decree of the Constituent Assembly had insured the payment of his pension. The decree was of no value, because the depreciation of the paper currency was sufficient to render it illusory. He had been named member of the *Board of Consultation*, appointed to examine and reward useful inventions. He had been appointed one of the administrators of the Mint. This commission offered him few objects to fix his attention, and could in no degree remove his apprehensions. It was again proposed to draw him to Berlin, and to restore him to his former situation. He had agreed to the proposal. Herault de Sechelles, to whom he had applied for a passport, offered him, for the greater security, a mission to Prussia. Madame Lagrange would not consent to quit her country. This repugnance, which at that time he considered as a misfortune, was to him a source of fortune and of new glory.

The Normal School, of which he was named Professor, but which had only an ephemeral existence, scarcely gave him time to explain his ideas respecting the foundation of arithmetic and algebra, and their application to geometry.

The Polytechnic School, the result of a happier idea, had likewise a more durable success: and among the best effects which it produced, we may place that of having restored Lagrange to Analysis. It was there that he had an opportunity of developing those ideas, the germ of which was to be found in two memoirs that he had published in 1772, and the object of which was to explain the true metaphysics of the differential and integral calculus. To render these happy developements more easily understood, the professor associated himself with his pupils. It was then that he composed his Analytical Functions, and his Lectures on that Calculus, of which he published several editions. "Those who have it in their power to attend these interesting lectures," said publicly one of the professors (M. Lacroix), "have the pleasure to see him create before the eyes of his audience almost the whole of his theory, and will carefully preserve several variations which the historian of the science will collect as examples of the path followed in analysis by the genius of invention."

It was then likewise that he published his treatise on the numerical solution of equations, with notes on the several points of the theory of algebraic equations.

It is said that Archimedes, whose great reputation, at least with the historians, is founded upon the machines of all kinds, by means of which he retarded the taking of Syracuse, despised these mechanical inventions, on which he wrote nothing, and placed importance only in his works of pure theory. We may sometimes conceive that the great mathematicians of our age entertained the same sentiments with Archimedes. They consider a problem as solved when it presents no analytical difficulty, when nothing remains to be done but differentiations, substitutions, and reductions, operations which require merely patience, and a certain dexterity derived from practice. Satisfied with having removed all the real difficulties, they concern themselves perhaps too little with the embarrassments which they leave to the calculator, and with the long labour necessary in order to make use of their formula, even after it has been suitably reduced. M. Lagrange had more than once attempted to abridge the usual calculations.

The general resolution of algebraic equations is subject to difficulties which are considered as insurmountable; but in practice every determinate problem brings us to an equation, all the coefficients of which are given in numbers. It would be sufficient therefore to have a sure method of finding all the roots of such an equation, which is called *numerical.* This was the object which M. Lagrange proposed to himself. He analyses all the known methods, and shows their uncertainty and insufficiency. He reduces the problem to the determination of a quantity smaller than the smallest difference between the roots. This is something. We cannot too much admire the analytical skill displayed throughout the whole work. But notwithstanding all the resources of the genius of M. Lagrange, we cannot conceal that the labour of his method is exceedingly great, and calculators will doubtless continue to prefer methods less direct indeed, but more expeditious. The author resumed this subject no less than four times. It is to be feared that a commodious and general solution will never be discovered, or at least it must be sought for by other means. The author seems to have acknowledged this himself, as he recommends the method of M. Budan as the most convenient and elegant for resolving equations whose roots are all real.

The desire of multiplying useful applications induced him to undertage a new edition of the Mecanique Analytique. His project was to develope the most useful parts of it. He laboured at it with all the ardour and intellectual power which he could have applied at any period of his life. But this application occasioned a degree of fatigue which threw him into a fainting fit. He was found in that state by Madame Lagrange. His head in falling had struck against the corner of a table, and this shock had not restored him to his senses. This was a warning to take more care of himself. He thought so at first; but he was too anxious to finish his work, the printing of which is at present at the twenty-sixth sheet of the second volume. The first volume had appeared some time before his death. It had been followed by a new edition of his Fonctions Analytiques. So much labour exhausted him. Towards the end of March a fever came on, he lost his appetite, his sleep was uneasy, and his waking was accompanied by alarming swoonings. He perceived his danger; but, preserving his undisturbable serenity, he studied what passed within him, and, as if he were assisting at a great and uncommon experiment, he bestowed all his attention on it. His remarks have not been lost. Friendship conducted to his house on the 8th of April, in the morning, MM. Lacepede, Monge, and Chaptal, who took care to write down the principal points of a conversation, which was his last. (We have scrupulously followed these notes, and the passages under inverted commas are faithfully copied from the manuscript of M. Chaptal.)

"He received them with tenderness and cordiality. I was very ill, my friends," said he, "the day before yesterday; I perceived myself dying, my body became weaker, my moral and physical powers were gradually declining; I observed with pleasure the gradual diminution of my strength, and I arrived at the point without pain, without regret, and by a very gentle declivity. Death is not to be feared, and when it comes without violence it is a last function, which is neither painful nor disageeable." Then he explained to them his ideas respecting life, the seat of which he considered as spread over the whole body, in every organ and all parts of the machine, which in his case became equally feebler in every part by the same degrees. "A little longer, and there would have been no functions, death would have overspread the whole body, for death is merely the absolute repose of the body; I wished to die," added he with greater force; "I found a pleasure in it; but my wife did not wish it. I should have preferred at that time a wife less kind, less eager to restore my strength, and who would have allowed me gently to have finished my career.

I have performed my task, I have acquired some celebrity in the mathematics, I have hated nobody, I have done no ill; it is now proper to finish."

As he was very animated, especially at these last words, his friends, notwithstanding the interest, with which they listened to him, proposed to retire. He retained them, began to relate to them the history of his life, of his labours, of his success, of his residence at Berlin, where he had often told us what he had seen near a King; of his arrival at Paris, the tranquillity he had enjoyed at first, the anxiety occasioned to him by the Revolution, and how he had been finally rewarded by a powerful monarch, capable of appreciating his worth, who had loaded him with honours and dignities, and who had even lately sent him the Grand Ribbon of the Imperial Order of Re-union. Let us add likewise, who after having given him during his life the most unequivocal proofs of the highest esteem, has since done more for his widow and his brother than ever Frederic had done for him while he was Director of his Academy.

He had neither been ambitious of riches nor honour; but he had received both with respectful gratitude, and rejoiced at the acquisition for the advantage of the sciences. He meant to affix these titles to the fronticepiece of his work, "in order to show the universe to what a degree the Emperor loved and honoured philosophers."

From these last words we see that he had not lost all hope of cure; he believed only that his convalescence would be long. He offered, when he recovered his strength, to go and dine at M. Lacepede's country house with MM. Monge and Chaptal, and proposed to give them details respecting his life which could be found nowhere else. These details are irretrievably lost. We do not even know to what he alluded, nor what he could have added to the second volume of the Mecanique Analytique, which was then in the press. We have just learned that the Countess Lagrange has put into the hands of M. Prony the complete manuscript of the second volume, in which will be found important additions, and sections entirely written anew. By the care of an editor so skilful, and so devoted to the memory of the author, the philosophical world is sure of obtaining with the greatest accuracy and despatch what is wanting to complete the work, and perhaps even memoirs entirely new.

"During this conversation, which lasted more than two hours, his memory often failed him; he made vain efforts to recover names and dates, but his discourse was always connected, full of strong thoughts and bold expressions." This exercise of his faculties wasted the whole remains of his strength. Scarcely had his friends left him, when he fell into a fainting fit, and he died two days after, on the 10th of April, 1813, at three quarters past nine o'clock in the morning.

M. Lagrange was of a delicate but good complexion. His tranquillity, his moderation, an austere and frugal regimen, from which he rarely deviated, prolonged his life to the age of seventy-seven years, two months, and ten days. He was twice married: first at Berlin, in order to be on a footing with the rest of the academicians, none of whom were bachelors. He brought from Turin one of his relations. He married her, and lost her after a long illness during which he had bestowed on her the most tender and unremitted care. When he afterwards married, in France, Mademoiselle Lemonnier, daughter of the celebrated astronomer of that name, he said to us, "I had no children by my first marriage; I do not know if I shall have them by my second; but I scarcely desire them." What he principally wished was an amiable companion, whose society might afford him some amusement during the intervals of his studies, and in this respect he was very successful. Madame Lagrange, daughter, grandaughter, and niece, of members of the Academy of Sciences, was deserving of the name which he gave her. This advantage in her eyes making up for the difference of their ages, she soon felt for him the tenderest regard. He was so grateful that he could scarcely bear to be separated from her, and it was on her account alone that he felt any regret at relinquishing this life; and he was often heard to say, that of all his good fortune, that which he prized the most was having obtained a companion so tender and attached to him. During the ten days that his illness lasted she never quitted him for a moment, and was constantly employed in recruiting his strength and prolonging his existence.

He loved retirement; but did not insist upon his young wife's following his example. On her account he went out more frequently, and indeed his high situations obliged him to show himself in the world. It was often apparent that he continued the meditations in public which he had begun in his cabinet. It has been said that he was not insensible to the charms of music. In fact, in a numerous company he was not displeased at a concert. On one of these occasion I asked him what he thought of the music: "I love it," says he, "because it leaves me to myself. I listen to it during the first three measures, but I hear no more of it; I give myself up to reflection, nothing interrupts me, and in this way I have solved many a difficult problem." Hence the finest music must have been that during which he was inspired with the finest of his thoughts.

Though he had a venerable figure, indicating his excellent characteristics, he would never allow his portrait to be drawn. More than once, by a very excusable piece of address, persons have been introduced during the meeting of the Institute to take a sketch of him without his knowledge. An artist sent by the Academy of Turin drew in this manner the outline from which was constructed the bust that was exhibited for some months in the hall of the Institute, and is at present in the library. A cast was taken of him after his death; and some time before, while he slept, a picture of him was taken, which is said to resemble him very much.

Gentle and even timid in conversation, he took a pleasure in asking questions, either to draw out others, or to add their reflections to his own vast knowledge. When he spoke, it was always in a tone of doubt, and his first words usually were, *I do not know.* He respected the opinions of others, and was very far from laying down his own as a rule. Yet it was not easy to make him change them. Sometimes he even defended them with a degree of heat which continued to increase till he was sensible of some alteration in himself; then he immediately resumed his usual tranquillity. One day, after a discussion of this kind, M. Lagrange having left the room, Borda remaining alone with me, allowed these words to escape him: "I am sorry to say it of a man like M. Lagrange, but I do not know a more obstinate person." If Borda had gone away first, Lagrange might have said to me as much of our associate, who was a man of excellent sense and considerable wit; but who, like Lagrange, did not easily abandon those opinions which he had adopted after a mature examination.

A gentle and good-natured irony was often remarkable in the tone of his voice; but I never saw any person hurt at it; because it was necessary to have well understood every thing that went before to perceive the true intention of it.

Among all the master-pieces which we owe to his genius, his Mecanique is certainly the most remarkable and the most important. The Fonctions Analytiques hold only the second place, notwithstanding the fruitfulness of the principal idea, and the beauty of the developments. A notation less commodious, and calculations more embarrassing, though more luminous, will prevent mathematicians from employing, except in certain difficult and doubtful cases, his symbols and names. It is sufficient that he has proved the legitimacy of the more expeditious processes of the differential and integral calculus. He has himself followed the ordinary notation in the second edition of his Mecanique.

This great work is entirely founded on the calculus of variations, of which he was the inventor. The whole flows from a single formula, and from a principle known before his time; but the whole utility of which was far from suspected. This sublime composition includes all his other preceding labours which could be connected with it. It is distinguished likewise by the philosophical spirit which reigns from one end of it to the other. It is likewise the best history of that part of the science, a history which could only have been written by a man perfectly master of his subject, and superior to all his predecessors, whose works he analyses. It forms a most interesting piece of reading even to him who is not capable of appreciating all the details. Such a reader will at least find the intimate connexion of all the principles on which the greatest mathematicians have founded their researches into mechanics. He will there see the geometrical law of the celestial motions deduced from simple mechanical and analytical considerations. From those problems, which serve to calculate the true system of the world, the author passes to questions more difficult, more complicated, and which belong to another order of things. These researches are only objects of pure curiosity, as the author announces, but they show the extent of his resources. Finally, we see there his new theory of the variations of arbitrary constant quantities of the motion of the planets, which had appeared with so much eclat in the Memoirs of the Institute, where it had shown that the author, at the age of seventy-five, had not sunk from the rank which he had filled for so long a time in the opinions of all mathematicians.

In every part of his writings, when he makes use of an important theorem, he names the original discoverer of it.

When he opposes the ideas of his predecessors and contemporaries, it is with all the attention due to genius; when he points out the errors of those who have attacked him, it is with the apathy of a true mathematician, and the calmness of a demonstration. None of his celebrated rivals had ideas more just, more fine, more general, and more profound. Finally, thanks to his happy labours, the science of mathematics is now like a vast and fine palace, the foundations of which he renewed, and in which we cannot take a single step without perceiving admirable monuments of his genius.

ART. XXV.—*Some Account of the Climate &c. of the North of France, collected partly from Observation, partly from a free Communication with the Inhabitants of various ranks.* Written, during a Residence in that Country, for the use of a Friend in Britain. By H. H. BLACKADDER, Esq. Surgeon. [*Brewster's Journal.*]

THE northern part of France distinguished more exclusively by the name of French Flanders, comprehends about 60 square leagues, or 540 square miles,—extending N. E. to W., from Dunkirk to Calais, about nine leagues, and N. to S. E., from Gravelines to Cassel, about eight leagues. This tract of country forms one extensive level plain, which was, beyond all doubt, at some remote period occupied by the sea. The insulated hill on which Cassel stands, and which rises from five to six hundred feet above the level of the plain, was at one time bathed by the sea, from which it is now distant about six leagues,—and there are monuments which seem to indicate that vessels could in former times reach even to St Omer, which is eight leagues from the present shore of the English Channel.

The period, at which the land began thus to encroach upon the sea, is lost in the lapse of ages,—all that we can discover from history is, that this encroachment had made considerable progress anterior to the age of Julius Cæsar. No fossil shells are found in the soil, but trees, similar to those found in Holland, have been dug out sixteen feet below the surface. The cause of this formation of land, and consequent repulsion of the sea, seems to be the successive accumulation of alluvial matter brought down by the rivers,—including, perhaps, the Rhine, the Meuse, and the Scheldt, along with the action of the North Sea on the coast of Holland. Gravelines, built at the mouth of a river, was, at the beginning of the last century, bathed by the sea, and is now distant from it about three miles.

The surface of the country is but very little elevated above the level of the sea at low water; and when the sea is full, the land is so much below it, that were it not for the artificial embankments along the shore, and if the sluices were not regularly shut at Calais, Gravelines, Dunkirk and Fort Nieulay, the whole country would be inundated. Hence it is that the rivers and canals do not flow during high water, a phenomenon which always attracts the attention of stangers, more especially when unacquainted with the cause.

Strictly speaking, there is but one river that passes through this country, which is named the Aa, and which has its origin near Reuti in Artois, about six leagues to the S. W. of St Omer. Passing through the last mentioned town, its direction is towards Gravelines, and about a league beyond that place it reaches the sea,—its whole course not being less than sixty or seventy miles. At different parts, as in the vicinity of St Omer, the bed of the river has been raised many feet above the surrounding fields, so that it is impossible to divine what its original distribution may have been. At Waten it gives off a branch called the Colme, which runs N., passes Bergue, and enters the sea at Dunkirk. A second branch goes towards Bourbourg, which it surrounds and crosses, and, with the Colme, reaches the sea at Dunkirk. About two centuries ago these branches were enlarged, (being then only small streams,) so as to form two canals for the purpose of navigation,—and serving also more effectually to carry off the water from the innumerable small canals, (termed *watergangs* in the language of the country,) and from the ditches which communicate with them.

To a stranger, nothing is more novel and characterestic, than the manner in which this tract of country is divided and intersected by the small canals and ditches ; the meadows, corn-fields, plantations, and gardens of each proprietor, being divided by these watergangs into small and more or less regular squares, each of which is completely insulated, so as to admit of boats readily passing between them. The whole may be compared to the streets and alleys of a large city, where water and boats come in the place of pavements and carriages. The execution of these works has been of incalculable advantage to the country,—immense tracts of rich soil, otherwise worse than useless, has been rendered highly productive ; land carriage is almost unknown ; and last, but not least, the endemic diseases, which were formerly the scourge of the inhabitants, have now nearly disappeared.

The river Aa runs slowly on a slimy channel, and which is fit for forming peat.* Hence the water has a peculiar brackish taste, which is very disagreeable. Over the whole of this country, indeed, the water is sufficiently bad; but that of the Colme and the canals is even worse than that of the Aa. In general it has a muddy brown colour, and deposits a greenish yellow slime in great abundance. Many of the inhabitants are nevertheless under the necessity of using water from the watergangs and ditches, however unwholesome. It is very dangerous to make use of these waters as drink, but their offensive taste commonly prevents their use in that form, the drink of the inhabitants being always a kind of tea or beer. Much use is made of water from draw-wells, though the quality of this is also very bad, being strongly impregnated with saline and other substances. It is more discoloured, and of greater specific gravity, than the water of the rivers and canals, and is unfit for culinary purposes, animal and vegetable substances when concocted in it, acquiring a horny consistence. The principal use to which the water of draw-wells is applied is the washing of the houses, a practice to which the inhabitants are, to appearance, excessively addicted. But, perhaps, if their houses were not thus kept proportionably damp, the inmates would be less able to resist the excessive dampness of the external air, to which they are liable to be exposed in all variety of circumstances.

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* In many places peats are dug out of the watergangs, &c. several feet under the surface of the water. For this purpose, the person goes into a large half-decked boat, and is provided with a very long-handled spade, the rest for the foot being four or five feet from the cutting extremity. With this instrument he digs up the peat, which is easily raised to the surface of the water, but then requires a considerable exertion to place it on the deck. Afterwards the peats are transferred to the land, to be thoroughly dried, and where, for a considerable time, they emit a most offensive odour. When dry they are very different from the peat to be met with in Scotland. They are very hard, black, and ponderous,—not easily kindled, and in burning, give out a most disagreeable and almost unsupportable odour. This odour is well known to strangers who may have walked round the old ramparts of Calais, though they may have remained ignorant of its true origin. The cause is doubtless

It is much to be regretted that cisterns, from which so much advantage has long been derived in Holland, are here almost unknown.

The soil seems to be of the same description over the whole of this tract of country. It is very moist, and is composed of a mixture of clay, silicious earth, peat-moss, and a considerable proportion of animal and vegetable substances in a state of decomposition. So much do the latter substances abound, that, when it is examined in the hand, it has that over-rich, or rather greasy appearance of the soil, that is to be found in the vicinity of old dunghills. Hence it is that ashes are in such great repute as a manure, and are sold at a high price. The cultivated fields are highly productive, and the pastures and meadows, especially those on the banks of the canals, around the villages and large farmhouses, are rich even to luxuriance. There are also considerable plantations, the vegetation of which is strikingly rapid, many trees acquiring large dimensions in the course of a few years. The progress made after engrafting is also remarkable; in some in-

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to be traced to the great quantity of decayed animal matter that enters into the composition of this species of peat. As the canals afford a ready and cheap mode of conveyance, and as both wood and pit-coal are high priced, these peats are much used by the lower ranks, both in the towns and villages, as well as in the country.

stances, indeed, that I had an opportunity of observing, it exceeded any thing I could have imagined in so northerly a climate. It may be observed, that the trees incline to the east, and that on their sides next to the sea there are comparatively few branches. The agricultural productions are, in general, of the best quality; wheat and rye in great abundance, a great quantity of these being sent to the interior. The diseases which affect the corn, are said to be le charbon, la nielle, la rouille, le blé moucheté; the ergot of the rye is of very rare occurrence. It is not uncommon, excepting in the summer months, to see the meadows, pastures, and, in some places, even the cornfields, covered with water. Hence it happens, that when the autumn is particularly rainy, the sown fields are ruined, and it becomes necessary to re-sow them in the month of March. Potatoes are in general cultivation, though not so much used as in Britain, excepting on occasions of scarcity. There are some varieties to be met with in the markets. The first is characterized by the paleness of the skin and whiteness of the flesh, resembling in these respects the variety named *white blooms* in some parts of Scotland, but in other respects it is much superior. It is, upon the whole, rather tasteless, and is most apt to be injured by frost. The skin of the second has a fine claret colour, and the flesh, sufficiently dry, is more compact than that of most other varieties. It seldom exceeds the size of an egg, has an agreeable flavour, and is much used in soup along with other vegetables. The third is an excellent and beautiful variety of the kidney species. The skin is of a bright claret colour, which extends into the flesh. It grows from two to five or six inches in length, and of uniform thickness, which is seldom so much as an inch. This potatoe is very dry, has a very agreeable flavour, and though, with other varieties of the kidney species, it is not very productive, it is surprising that it is not met with in our gardens. It is usually the first that appears in the market, and must be considered as an early variety. The fourth is that named the black potatoe, though its flesh be the whitest of all. This, as elsewhere, is cultivated for spring use, being universally recognised as withstanding the frost, and as being later in vegetating than any other variety. I never observed any appearance of disease in the potatoes raised in this country.

The horticultural productions are very abundant, and, upon the whole, rather luxuriant, approaching to that which is termed *rank*—the quantity and size being much more characteristic than the richness of the flavour. Still, however, the cherries, currants, apples, pears, peaches, plums, and other fruits, form a great part of the wealth of the inhabitants. Gooseberries are not much cultivated, and are chiefly used in an unripe state. Much use is made of roasted apples and pears, more especially in the time of Lent. The coarsest apples and pears, which are otherwise only fit for making cider and perry, become pulpy, and of a fine subacid taste, by simple roasting in the oven. In this state they are preserved close packed in jars for almost any length of time, and without the addition of sugar. Spinage and sorrel are also preserved in jars, so as to furnish a constant supply at a trifling expense during winter and spring,—a little salt and half boiling of the leaves being all that is necessary. For the purpose of promoting and protecting early vegetation, I find much advantage is derived from the use of easily moveable hurdles, made by placing a thin layer of straw or reeds longitudinally between thin wickerwork, and which can be constructed at almost no expense. These are placed in various directions around the beds, and shifted according to circumstances. There is one method of having early salad, which is very simple. They sow the lettuce seed very thick, and when the young plants are about an inch in height, they are thinned out. These young plants make a most delicate salad, and the taste can be rendered more piquant by the addition of a few plants of the *Lactuca virosa*, or by a little endive, or blanched dandelion.

On the sea-shore is found the *Critim. marit. maj.*, which, when confected in vinegar, is an article of commerce.

The prevailing quality of the air is humidity, the face of the sky being commonly concealed by a sombre greyness of the atmosphere, at no great height above the earth. The barometer is variable, seldom remaining of the same height for two days in succession. The extremes are said to be 26, 3^{l}—29. It has been remarked, that the changes are more frequent and more rapid than in the interior, and that the variations are most remarkable about the equinoxes. The mean temperature is about 5° above that of Paris. The cold season is of long duration, but is commonly not severe. Judging from the animal sensations, however, this climate is colder than that of Paris, which must arise from the great dampness of the air, caused by the vicinity of the sea, the number of canals and ditches, the lowness and consequent moistness of the soil, and the great luxuriance of vegetation.

When the south or west winds continue for some time in any season, abundant rain is the consequence. The animal system then feels relaxed and oppressed; the circulation is slow, and the secretions interrupted. Persons feel themselves heavy, and incapacitated for either mental or corporeal exertion, and many, but particularly strangers, acquire a dingy yellow colour of the skin, obviously connected with some irritation of the biliary organs. The state of the atmosphere is quickly changed by a north or northeast wind coming from the sea. These winds are dry and invigorating, but not unfrequently accompanied by a dense fog. The winds are very variable, often changing in the course of the same day. The most frequent are the southwest and the north, the most rare the east. The southwest and north winds are always the strongest, and the south wind is very moist. The southwest wind is common in autumn, the north wind in spring.

The number of serene days is about forty, and occur only during a very hot or very cold state of the air. Most commonly the sky is covered with a universal greyness, through which the sun only occasionally makes his appearance.

They count, on an average, about twelve thunder storms in the course of the year; and not unusually a tremendous one all at once ushers in the winter. In a storm of this kind which I witnessed, a wind-mill, at a short distance, was struck by the electric fluid, and immediately after the flash, there suddenly fell a shower of angular pieces of ice, by which some individuals had their faces slightly lacerated.

Heavy fogs are very frequent, often appearing at mid-day, and extending from the sea, envelope all Flanders, but seldom advance interiorly beyond the province of Artois. These fogs occur occasionally at all seasons, but are most common in spring and autumn.

The quantity of rain is only somewhat greater than at Paris. There, the mean is 24 inches 2 lines, and there are 119 days with rain; here, the mean is 24,4, and the number of days with rain 159. May, July, August, and November are the months in which most rain is observed to fall.

In spring, the north wind is almost constant, especially from the beginning of March to the middle of April. This wind is strong, dry, cold, and sharp. It freezes in the night, and the barometer always keeps high, but the sky is uniformly obscured. Hoar-frosts are apt to occur until May; and it is not till the middle of that month that the air begins to feel sensibly and steadily warmer.

In summer, the weather is very variable, there being continual changes from heat to cold, and *vice versa*, not only from day to day, but in the course of the same day. A fog coming in a direction from the sea, extends all over the country, and then the thermometer sinks. In July and August the heat is considerable, sometimes so high as 92°. On such occasions there is always a perfect calm, but this state of the air never lasts long; clouds soon begin to make their appearance, and the thermometer falls to about

65°. The usual summer heat is from 65° to 80°. At this season there is always a heavy dew during the night, accompanied by a very cold and dense fog, the thermometer falling from 8° to 14° lower than during the day. Hence the origin of various diseases, to which persons are liable when exposed to the open air during the night. No where is the sudden sensation of cold at sunset more remarkable.

In autumn, the weather is more steady; after the equinox, the thermometer sinks to 50°, and vacillates but little for a considerable length of time. Frosts do not usually commence till December; but during the night and part of the morning, there is always a dense cold fog, which moistens all the surface of the ground, and of solid bodies resting on it.

In winter, the temperature rarely falls so low as 20°, but it has once or twice been observed nearly at 0°. In the winter of 1819, I observed it so low as 10° a considerable time after sunrise. There had previously been a great fall of snow without wind, and there was now a gentle but steady northerly breeze. The whole sky was free of clouds, had a deep blue colour, and a peculiar glistening appearance, produced by a minute icy spiculæ with which it was heavily charged, and which, impinging on the face, in walking against the wind, were sufficient to blister it. These spiculæ had no effect on the appearance of the sun, if it was not to render the light more intensely white and glistening.

A humid atmosphere, habitual heavy fogs, a moderate temperature, a low and moist soil, the free use of watery drinks, such as beer, the daily use of a great quantity of milk and butter, an abundant nourishment of wheat-bread, butcher's meat, tea, and fresh water fish, and garden vegetables of all kinds, the ease of all ranks of society, and labour to occupy all hands, all these things united must greatly influence the physical and intellectual constitution of the inhabitants. In general, men, animals, and vegetables, are large. A moist climate, and a fertile soil, are favourable to the developement of all the parts of animate beings. Hence we remark large bones, large muscles, with a profusion of cellular substance. When King Henry the VIII. described a certain princess as "a great Flanders mare," the comparison was rude, but sufficiently intelligible. In general, the complexion of the inhabitants is fair, the eyes blue, and the hair chesnut. They support labour well, and often arrive to an advanced age. In character they are a mixture of the Frenchman and Hollander, and which, when closely anlayzed, is found to be rather an odd compound.

Beer of various qualities is much used, and spirituous liquors (particularly gin, which sells at the rate of about sixpence the bottle) are consumed in great quantity. Drunkenness, however, even in the towns, is rare, very rare, compared with what may be observed in Britain, and habitual drinkers still more so. But it is an almost universal practice to take a glass of raw spirits on first rising in the morning, a practice common, and said to have been found beneficial, in most damp countries on the continent of Europe. The use of spirits mixed with water, in the form of grog or punch, is all but unknown, such a mixture being considered highly pernicious to the stomach, and it would be well, perhaps, if all their opinions were equally well founded. Tea, with or without milk, but always without sugar, a weak beer, and "bouillie," are the common drinks. As formerly mentioned, the water cannot be used in its crude state with impunity. The bouillie is made from a decoction of bran, to which old leaven has been added ; fermentation ensues, and the product is a refreshing subacid liquor, which removes thirst very well.

Milk, butter, cheese, and bread, constitute the principal nourishment of the country people. Even the common people never eat bread without butter (excepting in Lent), and he who is reduced to do so, is considered at the depth of misery. Hence the proverbial expression, "*Manger le pain sec.*"

There is a mess called sour-milk much used by the country people, and which is made as follows : A considerable quantity of milk is put into a deep wooden vessel, and a certain quantity of salt is added to it. It is then left until the whey separates from the curd, when the former is poured off and given to the pigs, and the latter is stirred round, and more milk added to it. This operation is repeated until the desired quantity of curd is obtained, and which is found to have acquired a very acid taste. In this state it is kept for winter use, and is used in mixing a quantity of it with water and flour, which is boiled, and then bread is added to it. This mess is used for breakfast and supper, which always concludes with bread and butter.

There is another mess of curd often used in the summer months, and much relished for supper. For making it, two vessels are provided, the one of which goes within the other, the innermost being perforated with numerous holes for the escape of the whey. The milk is coagulated by means of runnet made by infusing a small piece of the dried stomach of a young hare in white wine. To the curd, well freed of the whey, is added salt and pepper, but many consider eschalots an indispensable ingredient.

In the country the houses are built of mud or brick, and roofed with thatch. In the towns, they are built of brick, and roofed with flat tiles, which at least have a better appearance than those used in Scotland, but they require the roof to be covered with deal, the same as in the case of slates. Nothing is more agreeable than the internal appearance of these houses; every where there is exhibited, as in Holland, an exact attention to cleanliness; but the means employed for attaining this is far from being agreeable to strangers. Several large tubs of cold water are emptied in each apartment, and when every part has been well scrubbed with a birchen broom, the remaining water is swept out by the door, and in this state it is left to dry, which, in this climate, is a very slow process. This deluging operation is of perpetual recurrence, and hence those who are not completely accustomed to this moist manner of life, are continually exposed to attacks of rheumatism, toothach, &c.

Occasional inundations from heavy falls of rain are liable to take place, and when the water is nearly evaporated, there are elicited vapours of a very noxious quality. Notwithstanding the numerous canals, &c. there still exist some marshes, and in hot weather their effluvia are very pernicious. Some of these marshes are formed by digging peat, and clay to make bricks; others have existed from time immemorial. At each farm-house there is a pond of no small extent, to receive the fluid part of the dunghills, &c. These are of a greenish or reddish brown colour, and in autumn give out a most abominable odour. Strange as it may appear, however, almost every farmer sends his cattle to drink out of these ponds, and even the animals themselves seem to prefer this fluid (for water it cannot be called) to the water of the canals and ditches. It must be admitted, that there are no direct proofs of the use of this fluid as drink, being prejudicial, and one thing is certain, that these ponds must be almost, if not altogether free, of insects and animalculæ, which literally swarm every where else, whether in air, earth, or water.

The immense quantity of small animals of one kind and another, that are to be seen in and about the canals, ditches, and fields, enables us to account for no small portion of the decayed animal matter that is so abundantly mixed with the soil. Frogs are very abundant, but though they are regularly brought to the market in Brussels, I never saw or could hear of their being used as food in this country. At some places leeches are bred in the marshes, and during the war, a great deal of money was made by the sale of them. A common mode of collecting these animals deserves to be noticed, as it explains a fact which has often caused suspicion and blame to attach to upright leech-dealers in the interior, and in Scotland not less than elsewhere. A number of persons, commonly young women, are employed to wade in the water with their feet and legs uncovered, and when they come out after a short stay, these parts of their body are found covered with leeches, which are then removed and secured in vessels provided for their reception. This operation they repeat as often as may be requisite for obtaining the desired quantity, and, far from suffering any injury from the loss of blood, these Flemish water nymphs informed me that their health was always improved by it at the time; that their complexions became clearer, and that afterwards they got more into a state of *em bon point*. It must be obvious, that leeches, caught in this manner, must acquire a small quantity of blood, but when afterwards they are sold for use, and when some of this blood, still remaining, happens to be ejected on the application of the animal, it is forthwith taken for granted, that the dealer has acted dishonestly in selling used leeches for fresh ones.

Hares, partridges, and snipes, are in great plenty. Quails were once common, but are now become exceedingly rare. In the river are trout, which I have seen from two to three pounds weight, but their flesh is not delicate, approaching somewhat to that of the pike, which is, no doubt, owing to the gross slimy quality of the water. Artificial flies for the purpose of fishing are here unknown; —an excellent proof this that necessity is the mother of invention, for here artificial flies are unnecessary. At almost every step along the river is to be found the fly most proper for ensnaring the trout, and as it is neither small nor delicate, and as the river moves slowly, it is not soon torn from the hook. The fly is often seen to rest on and float down the stream when seized by the fish, and the fisher has only to imitate this dull operation. In the canals and ditches are pike, and an abundance of perch and other small fish. I have been informed that, about twenty years ago, a pike was killed of the enormous weight of thirty-two pounds; no one seemed to doubt the fact, but I have met with no one who had himself seen such a fish.

The canals and ditches, from time to time, become more or less choked, from the continual deposition of alluvial matter, and, when cleared out by the workmen, the matter ejected yields a most unsupportable odour. As the riches of the country depend on the keeping of these canals in repair, they are cleaned out at least once every two years, besides other occasional repairs; and there is a class of workmen who nearly confine themselves to this particular occupation, which is none of the most healthy. It is in autumn, winter, and spring, that these operations are carried forward, and the matter ejected is spread over the fields as manure. Nothing of the kind can be more offensive to the smell, and few more pernicious to the health, than the vapour which continues for a long time to exhale from it. Still, if we suppose a foreign army to take possession of this country, it would be of the first importance to protect the inhabitants, and give every possible encouragement to the regular execution of these works, otherwise they would soon and dearly pay for their imprudent conduct.

On the sea-shore the inhabitants are, in general, more healthy than in the interior; but when the sea happens to break over the dikes, and the water afterwards comes to be evaporated, exhalations of an extremely pernicious quality are elicited, and which cause fevers of the very worst kind.

Intermittent fevers are not so frequent here as on the coast of France, in the vicinity of Rochfort. In this country the ditches are less frequently dry, the temperature being considerably lower, and the air in common much damper, the evaporation is much less active. The adult inhabitants are but little subject to the attacks of the endemic fevers, but children and strangers rarely escape. They are commonly attacked in autumn; at first it appears under the form of a tertian, afterwards it changes to a quartan, and about January, some change to tripple quartans, and instances to quotidians. From the middle of April to the middle of May, these fevers, when not interfered with by medical treatment, cease of themselves. The patients, however, are liable to a recurrence of the disorder in the course of the ensuing summer; and nothing is more common than to see children who have had the fever hanging about them for two or three years in succession, during which time they preserve their appetite, and even eat more than when in perfect health. It is not customary to treat this disease medically in the case of children, and accidents are rare. When a child has once had and got quit of the fever, he is thereby *acclimaté*, or rendered less liable to suffer from the climate in the after period of his life, if he be not much exposed to the exciting cause, such as digging the ditches. Of all others, the men who mow the hay suffer most; the ditchers are all natives, but the hay-makers come from a different and higher part of the country, being allured by the love of gain. As they always work at what is termed piece-work, their exertions are great, and as the hay falls, the dank surface of the soil, consisting of the half decomposed ejectment from the diches, is exposed to the direct rays of the sun: Hence it happens that the noxious vapour arises most abundantly at a time when the men are exhausted with labour, and consequently when the system is least able to resist its influence. These labourers contract complicated tertians, and, in the language of the country, are said to have "*swallowed the frog in Flanders.*" The patient experiences a feeling of great debility; the secretions seem almost suspended; the respiration is slow and oppressed; and the surface of the body assumes a leaden colour. The first care of these unfortunate persons is to quit the marshy country, and when they get home, and are properly attended to, they often recover, though some occasionally sink under various supervening complications.

Upon the whole, this is a very rich and populous country, and in several respects, very highly interesting; but it is neither very agreeable nor very healthy, especially to strangers, as a place of residence. Catarrh and rheumatism are frequent in autumn and spring. In summer diarrhœa, verging to dysentery, is not unfrequent. In the towns, chronic ophthalmia is pretty common, and there appears to be an unusual proportion of halt and lame, and that apparently from their youth.

Art. XXVI. *Essay on Calico-printing.* [*Parkes' Chemical Essays.*]

As the whole of this ingenious business, as it is now conducted, depends upon the proper application of a few compounds called mordants, it will be necessary, in the first place, to explain their nature and uses. In doing this, one or two preliminary remarks will assist us.

The colouring substances chiefly employed in this art, are divided into two classes, viz. *substantive* and *adjective*. A SUBSTANTIVE colour is one which is capable of itself of producing a permanent dye; such is the juice of the buccinum,[*] used by the ancients for producing the imperial purple; such are also the woad and indigo[†] employed by the moderns for producing a permanent blue; and we may add the metallic solutions, particularly those of iron, cobalt, gold, platinum, and silver, which give various colours, according to the processes by which they are prepared.

By ADJECTIVE colours are meant all those which are incapable of giving permanent dyes without the aid of certain intermedia, which form as it were a bond of union between them and the substances intended to be dyed.

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[*] It has been proposed to employ this valuable permanent colour for penceling on fine muslins. I believe it might readily be procured in sufficient quantities, and would prove an important addition to the resources of the British calico-printer.

[†] Dr Bancroft tells us, that the first mention of indigo, as known in England, is in the Act of the 23d of Queen Elizabeth, chap. 9, where it is called *Ancle*, or *Blue Inde*. Bancroft *On permanent Colours*, page 138.

These intermedia are what are known by the term MORDANTS, and they are used for this purpose in very considerable quantities by the calico-printer of the present day.

"The ancient Gauls and Britons were not ignorant of the method of imparting various dyes by means of substantive colours; we have even direct evidence that they excelled in some branches of this art, and possessed valuable secrets in it that were unknown to other nations. Several of the herbs which the Gauls and Britons used in dyeing are occasionally mentioned by Pliny, in different places; but the herb which they chiefly used for this purpose was the *glastum* or *woad*, the isatis of Linnæus; and they seem to have been led to the discovery of its valuable properties in dyeing cloth, from the former use of it in painting and staining their bodies."*

Several expedients were also employed by the ancients to produce fast or, more properly, permanent colours, by means of mordants, as appears from the testimony of Aristotle and Pliny. The chief articles in use at present in Great Britain, are the acetate of iron, the acetate of alumina, and the various solutions of tin, all of which should be very carefully and correctly prepared.

When piece-goods are designed to be dyed of one uniform adjective colour, they are first immersed in a solution of one of these mordants and are then hung up to dry, and to absorb the oxygen of the atmosphere. When sufficiently exposed to the air, they are washed or dunged,† to remove the superfluous mordant; that is to say, that part of it which is not chemically combined with the cloth; and the pieces are then submitted to a bath of that particular kind of colouring matter which is to be imparted to them.

It may here be remarked, that if a sufficient number of colouring substances should ever be discovered, that have no affinity for any thing but the chemical mordants, the business of calico-printing would be rendered much more easy and simple than it is at present.

Whenever it is meant that a colour should be partially inserted, the mordant is applied to those particular parts only; so that, when the piece is immersed in the colouring bath, no other place will receive the permanent stain. For though the whole surface of the cloth will be coloured, yet having in itself no affinity with the vegetable with which the decoction is impregnated, the whole of the colouring matter will be easily removed by exposure to the air, and the ground of the piece restored to its original whiteness; while those parts to which the mordant was applied, will retain and fix the colours in a way which will be more fully explained hereafter.

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* Henry's *History of Great Britain*, 8vo. vol. ii. page 128.—Pliny's *Natural History*, books 16, 18, 21, and 22.

† The dung of the cow is used in such large quantities by the calico-printer, that it has become an article of great expense. The proportion that is employed is usually about one bushel to one hundred gallons of water, though frequently a larger proportion would be more effectual. The brightness of the colours, and the purity of the whites, are always dependent upon the quantity of the dung employed.

Formerly all calico-printers were bleachers; but in the neighbourhood of London these are separate and distinct trades, and the printer either purchases bleached goods for printing on his own private account, or receives the cloth from his customers in a white state; and, when printed, he returns the identical pieces, and is paid so much per yard, according to the number of colours, for printing them.

Chloride or oxymuriate of lime is the agent generally employed in bleaching; but it appears to me that some other article might be introduced with advantage. For, as the goods are washed in diluted sulphuric acid when they are taken from the oxymuriate of lime, a sulphate of lime is always formed, which becomes fixed in the fabric, and, acting as a mordant when the pieces come into the madder copper, occasions an indelible stain, which in very fine goods often impairs their beauty. If oxymuriate of soda were employed, the sulphuric acid would form a soluble salt with the soda, easily removeable by washing. The expense of this article will, however, be always an objection to its use, except for rich and valuable prints.

I am aware that some printers, who are more anxious about the quantity than the quality of the work they perform, would deride this caution, and say they can obviate the difficulty by washing the goods *before* they are submitted to the sulphuric acid. But I am so confident of the extreme difficulty there is, even in this way of removing the oxymuriate of lime, that I know the inconveniences above mentioned will very often occur, almost inevitably.

In my opinion every printer should bleach his own goods, for it is impossible always to rely with confidence on the care of those who bleach for hire; and every printer knows that good bleaching is absolutely a necessary preliminary in the production of good printing. Indeed, this is now pretty generally acknowledged in the north of England; for most of the opulent houses in Lancashire and in Scotland, who produce fine work, are bleachers as well as printers.

No people have taken more pains to excel in bleaching than the

Irish, and their credit is established accordingly. The German linen is, I believe, generally better than theirs; but the Irish has always the preference in foreign markets, owing to the superiority of the Irish in bleaching and finishing.

By whatever means the bleaching is performed, the printer commences his part of the business in the following manner.

The goods are first DRESSED by singing off the whole of the nap which is attached to them. This is effected by the following contrivance: Ten pieces are generally wired together, and wound upon a roller, from whence they are passed over a hot iron, nearly in the form of half a cylinder, and received upon another roller; from thence they are returned to the iron, which is still kept red or nearly at a white heat. The use of repeating this process is to remove the nap more effectually than it would be done by passing it only once over.

The next operation is that of STEEPING, which consists merely in soaking the pieces for twenty-four hours in a vessel of weak alkaline lye, at a temperature of about 100°. These operations of singing and steeping are going on at one and the same time, which effectually prevents any accident that might otherwise arise from the effects of the hot iron.

The goods are then boiled or else bowked in a solution of potash; (some workmen prefer to have this alkali in a pure caustic state;) they are then well cleansed, by thorough washing in wash-wheels, or in stocks, to ensure their being entirely divested of the alkali. The intention of thus treating them with potash, is to remove any grease or impurity that may be attached to them, which would otherwise endanger the evenness and uniformity of the colours. This process is called ASHING.

By some observant calico-printers it has been imagined, that the rendering the lye caustic is apt to impair the texture of the cloth; and I doubt not but that this has often been the case. Under the eye of the master, however, I am sure it might be employed with advantage and safety. With advantage, because *mild* alkali has but a slight affinity for the grease; whereas *caustic* alkali forms a real soap with any oleaginous matter, and then the compound is readily soluble in water. And though the alkali be made perfectly caustic, if the solution be sufficiently diluted, it may be used with perfect safety.

It may be remarked, that in weaving calicoes the workman generally greases the reeds, in order to make them move easier. Whenever this grease is in the cloth, it becomes fixed by the operation of singing; and if it be not taken out before bleaching, it will not come out afterwards by the usual process of ashing and souring; for, when the pieces are submitted to a blue vat to be dyed of a uniform self-colour, all those greasy places will be found to have taken the dye in a very imperfect manner. Tallow is also employed in dressing the warp, and this has a baneful effect on all goods which are designed for printing.

If, however, the calico-manufacturers themselves would make a point of preparing the oleaginous matter for the weavers, and would furnish them with nothing but pure VEGETABLE OILS, such as those of rape, linseed, &c., I think it very likely that these inconveniences would not occur; because the stain from *vegetable* is not so indelible as that from *animal* oil.

This defect in the operation of the pale blue dipping is often attended with very serious consequences, so that I know of no subject which better deserves the patient investigation of the calico-printer. I have seen a large number of pieces of printed calicoes which had all taken a solution of iron so as to form an extremely good black; also the acetate of alumina, which had formed with the madder a beautiful red; likewise the citric acid, which produced a perfect resist—and yet, when these goods came into the blue vat, certain parts of the pattern intended for blue, still remained quite white, the indigo having had no action whatever on those parts; a circumstance which is sufficient of itself to spoil the goods, and render the whole expense of printing the other colours totally lost.

To cleanse such goods previously to their being printed, various expedients have been adopted, but I apprehend nothing but a solution of caustic alkali can be depended upon. To prove the effect of any method which may be tried, it is a good way to run the pieces through water, and then to pass them from the water so gradually over a roller, as to give the superintendant an opportunity of examining every inch of the surface; and if any part remains greasy, it will be seen at once, for that part will continue dry, while all the rest of the cloth is wet.

There is another way in which the goodness of bleaching might be proved. Let a few of the suspected pieces be run once or twice through a madder-copper, at the temperature of about 180°. This will inevitably mark any part that may be imperfectly bleached; whereas, if the operation has been properly performed, they will come out so little stained, that an intelligent workman, who has been used to a madder copper, will at once be satisfied that they contain no impurity that can form a permanent mordant.

The next process in calico-printing is one with diluted sulphuric acid. A quantity of soft water having been poured into a leaden vessel, oil of vitriol is gradually added to it, in the proportion of about twenty pounds of oil of vitriol to every hundred gallons of water, which by weight is in the proportion of about one to forty.

When this mixture has been well stirred, it is ready for use. Sometimes it is employed in this state, at others it is heated to 90° or upwards of Fahrenheit, according to the nature of the work to be done, and the goods are immersed in it. They are not suffered to lie in this acidulous liquor, but are wound by means of a winch over a wooden cylinder, that every part of the cloth may be successively immersed in the fluid, and every part exposed alternately to the action of the atmosphere.

This operation is generally continued for about twenty minutes, and is designed to remove any iron-moulds or other stains which the cloth may have acquired. It has also the effect of neutralizing any portion of potash that may have been left in contact with the cloth. The process is called SOURING.

After this operation it is necessary to wash the goods thoroughly, that no part of the acid may be left in them, to injure their texture; and this is best effected by means of the wash-wheel. The calicoes are then to be regularly and thoroughly dried, which finishes these preliminary operations, known in the trade by the term PREPARATION; so that those cloths which have passed through these manipulations are said to have undergone a preparation.

Besides the uses already mentioned, there is another advantage attending these processes, viz. that the cloth which has undergone this preparation will bleach sooner, the colours will be brighter, and the whites more delicate, than they would have been had they not gone through these previous operations.

The next process is that of CALENDERING. Here the goods are passed through a set of rollers, which gives them a gloss, and the appearance of their having been ironed. They are now fit for printing. But for copper-plate printing, or cylinder work, the process of calendering is omitted.

In printing fast colours, the artist usually proceeds in this way: He lays the piece of calico, which has been already smoothed by calendering, upon a strong thick table, which is previously covered with a woollen cloth. He then proceeds to apply one or more mordants, as the case may require, for fixing the intended colours. These mordants are applied by means of wooden blocks, with the patterns formed upon them. These blocks were formerly chosen of holly, or other hard wood, and the cutting them was a separate branch of the business. Of late years, however, a considerable improvement has been made in this department, by the introduction of brass or copper; that is, the pattern, instead of being actually cut in the wood, is now formed by means of slender pieces of one of those metals being firmly fixed to the block, so as to produce the pattern intended. This alteration was occasioned by the perishable nature of wood, on account of which every printer has incurred great and unnecessary expense. The pattern when thus formed with copper, is not only more lasting, but it has also the advantage of giving greater sharpness and beauty to the impression.

When it was customary to use wooden blocks, the patterns were not enchased in the wood, but the wood was cut away in such a manner as to leave the pattern in relief. It will be obvious that this must always be the case in *block*-printing.

When the mordant is ready, it is mixed up either with flour-paste, or with a thick acqueous solution of gum arabic, gum senegal, or gum tragacanth.

Flour is an article of considerable consumption with the printers for making paste. Some houses buy twenty barrels of American flour at once. Should it be musty or sour from keeping, it is of little consequence for their use ; but they are careful to buy none but such as has been made with sound wheat, for if unsound it will be of no value for their purposes. Gum tragacanth is much dearer than the other gums ; but notwithstanding this, it must be had for some styles of work, as no other will answer for any of those colours or mordants which are prepared with nitrous acid. A solution of gum senegal would be coagulated in an instant by any of those preparations. Of late years an article called British gum has also been much in use for the same purpose ; so much so that the making of it has become a distinct and considerable trade. It is merely common STARCH calcined till it assumes a cinnamon brown colour, and then pulverized and passed through sieves of various degrees of fineness.

When the colour has been properly mixed with flour paste, or with a solution of either of the gums above mentioned, it is then spread upon a piece of superfine woollen cloth, strained tight upon a hoop.

This is placed within another hoop, covered either with sheep-skin or oil-cloth. These hoops are both so broad as to give to each of them the appearance of a tambarine. That which is covered with the woollen cloth is called a *sieve*, the other a *case*. The sieve within its case is now placed in a small tub of gum-water, and is ready for use.

When the apparatus is thus prepared, the mordant is applied by a brush to the surface of the sieve. This is called TEERING.

It should have been remarked, that when a colourless mordant, like the acetate of alumina, is employed, the workman generally mixes a little of the decoction of Brazil wood, or of any other fugitive dye, with it. This is called SIGHTENING; and is for the purpose of making the pattern more obvious to the workman, that he may see its progress, and the efficacy of the materials, as he proceeds in printing. The manipulation may be thus described.

Taking the block containing the pattern in one hand, the workman applies it gently to the surface of the sieve, so that a sufficient quantity of the thickened mordant may adhere to the figures. When the block is thus charged, he applies it to the calico, and gives it a blow with a small mallet, either slightly or otherwise, according to the nature of the pattern.

This alternate application of the block to the sieve and to the calico, is continued until the workman has gone over the whole piece. In this way several different mordants are sometimes applied to the same piece of goods. This is indeed always necessary, when the finished piece is intended to contain a variety of colours, the different colours requiring different mordants to fix them and render them permanent.

The calico is now removed to a room called the stove, where a certain degree of heat is given to it by means of flues, which go round the room on the inside, near the floor. In this room it is generally continued for at least twenty-four hours.* The intention of this is to evaporate the acids used in the preparation of the mordants, and which might otherwise injure the texture, and also to fix the base more surely within the fibres of the cloth.

Whenever acetate of alumina is printed as a mordant upon calico, it appears to me that the pieces ought not to go into the stove until the work has been first thoroughly dried in the open air, because it is well known that the earth of alumina is precipitated from a solution of this salt in a high temperature. I suspect this to be the cause of many of those disappointments which are perpetually occurring in the production of the various shades of yellow and madder reds.

In the operation of stoving, an attention to temperature is of the utmost importance. In general the room is kept at about 90°; but an intelligent calico-printer varies this according to the nature of the work under operation.

It was formerly a practice to expose all those calicoes which were printed with the several mordants, to a hot stove for the purpose of evaporating the acids and fixing the base with more certainty in the fibres of the cloth; but of late years a more convenient method has been adopted, which consists in passing the calicoes by means of machinery along a set of apparatus known by the name of *steam-chests*. These are large square cast-iron vessels fixed at some little distance from each other, sometimes on an inclined plane, and in other cases perpendicularly one above the other ; these are kept constantly full of steam, and are connected with each other by bent pipes either of iron or lead. It is so contrived that the pieces of calico are made to pass over each of these heated chests in contact with their surfaces ; and in consequence of the access of atmospheric air, as they pass from one of these chests to the other, they are generally found to have become thoroughly dried by the time they have arrived at the end of the series. A better apparatus has however been lately invented, consisting of an arrangement of cylinders made of tinned iron or copper filled with steam, and round which the calico is made to pass in succession. In this apparatus there is a vane fixed underneath every two of these cylinders, which is moved by means of the steam-engine, and agitates the air so as very much to facilitate the drying process. This mode of drying printed calicoes is so effectual, that although the cloth be wound from a heap at one extremity of the apparatus upon the first cylinder quite in a wet state, it becomes thoroughly dry by the time it has passed over the whole apparatus, and falls in a perfect state on the heap at the other extremity of the apparatus.

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* This is when common red-liquor has alone been printed; but if citric acid or strong muriate of tin has been employed, less time is sufficient, and for the latter seldom more than half an hour is allowed.

If iron-liquor has been employed in printing the goods, it is an excellent practice to keep them for several days exposed to the atmosphere* after their removal from the stove, as the blacks, pompadours, olives, and indeed every other colour prepared with that metal, will increase in intensity; the goods will clean better in the dung-vessels, as will be explained hereafter, and the colours will rise higher and brighter when they come into the copper of bark or madder.

It may be worth an experiment to discover whether the colours containing iron would not be better if they were suffered to be only a *very short time* in the stove, but were hung up instead for several days, exposed to a current of air at the temperature of the atmosphere ; as the iron would thus acquire the oxygen slower, and consequently would be fixed more firmly in the fibres of the cloth.

When the pieces have been properly stoved, they are passed, by means of a winch, through water at various temperatures, with a little cow-dung mixed in it. The intention of the dung is to absorb and remove that portion of the mordant which is not actually combined with the cloth, and which otherwise might stain the white or unprinted parts.

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* The iron in an acetous solution is in the state of the *black* oxide ; but by exposure to the air it acquires a further dose of oxygen, and obtains the state of the *red* or peroxide.

This part of the business was formerly conducted in a very uncleanly and negligent way; but of late years some printers have incurred a considerable expense in the construction of their dunging machines, with cocks for hot and cold water attached to them, and thermometers to regulate the temperature.

I suspect, however, that the dung of the cow is serviceable in another way besides that of cleansing,* though few printers may be aware of the nature of its operation.—It is acknowledged that madder, cochineal, and some other dyes, produce much better colours on woollen than on cotton cloths, owing to the former being of animal, and the latter of vegetable origin. I presume, therefore, that the dung imparts an *animal* matter† to the fibres of the cotton, and that this animal matter acts as an additional mordant, and thus more powerfully attracts the colouring particles of the dye, than the mordants alone would be capable of doing.

If a piece of calico, prepared with the acetate of alumina be divided into two parts, and the superfluous mordant removed from the one by cow-dung and water, and from the other by water only, at the same temperature, it will be found, on passing the two portions through a decoction of weld or quercitron bark, that the yellow will be much more intense and bright in that which has been submitted to the action of the cow-dung.

The process of DUNGING calicoes is an operation that varies in time from five to forty minutes, according to the style of the work. The pieces are then taken to the river or wheel, to be more effectually washed; and after this they are passed through tepid water, in order that the workman may be assured that every impurity is removed.

His next care is to provide a copper boiler of pure cold water, into which a sufficient quantity of madder is broken, and then a fire is lighted underneath the copper. The calicoes, printed and rinsed as above, are now put into the boiler, and from the time they are immersed, the workman never ceases to turn the winch, so as to pass every part of the goods repeatedly through the liquor, until the whole acquires a boiling heat. Indeed, this operation is sometimes continued for ten or fifteen minutes after the bath of madder actually boils, when the pieces are taken out and washed thoroughly. In most of the large calico-works, a considerable improvement in the method of maddering has been adopted. This consists in giving a perpetual motion to the calico by means of the steam-engine. The ends of several pieces are fastened together, and then they are put into constant motion through the madder-vessel, whereby the colours are rendered more uniform; and one man is enabled to attend to three dyeing-vessels at one time, which is found to be a great economy of labour.

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* To *cleanse* calicoes by immersion in a dung-vessel, may appear to be a strange phrase; but as this is the technical language of the trade, no other term could be employed with propriety.

† Berthollet, who analyzed the dung of the cow, found in it a substance partaking of the nature of animal *bile*.

Madder is one of the most valuable drugs we have, for a variety of purposes in dyeing and calico-printing: as it is the agent by which the best and most permanent blacks are produced; also the finest purples, and every shade of red from a pale pink to a crimson. But perhaps it may not be generally known that this article improves by age. For instance: If a quantity of madder-roots be ground, and then packed tight in a cask, so as to exclude the air, and kept thus for six months, they will dye a much better colour, and go much further, than they would have done had these roots been used as soon as they were ground.

The process which is called MADDERING, has the effect of imparting all the requisite colours to the goods by means of *one* operation, which may be thus explained:

While one mordant precipitates the colouring matter of the madder to a red, another precipitates a different portion of it to a purple, another precipitates it black, and so of every possible shade, from a lilac to a black, and from a pink to a deep red.

If a portion of weld or bark be added to the madder, every shade from a brown to an orange may be produced; whereas, if weld or bark alone be employed, all colours between a dark olive and a bright lemon can be imparted to the cloth. These changes are all occasioned by the play of chemical affinities, for the knowledge of which we are indebted to the improved state of chemical science.

The only materials which are employed by the calico-printers for the production of fine yellow are the quercitron bark and the weld plant; the former is the bark of a peculiar kind of oak growing in America, the latter is the produce of our own country. The weld plant (the *reseda luteola* of Linnæus) is for the most part cultivated about Doncaster in Yorkshire. It is found, however, that nothing impoverishes the land so much; otherwise it produces a very heavy and profitable crop.

Here it may be worth remarking, that whenever it is of consequence to produce the finest yellows or more delicate lemon colour, it is necessary to dry the pieces in the open air, as the stove would not fail to injure such colours; for stove-drying has always a tendency to convert a yellow to an orange It is also necessary to be equally careful in the operation of dunging the mordants for these pale yellows; for should this be done at a higher temperature than 96° or 100°, their beauty will certainly be impaired. There is another advantage in this, viz. by dunging at this low temperature, the dyeing may be completed even at 110° or thereabouts, which will give a much livelier colour than where a higher temperature has been employed.

The mordants generally used in calico-printing, are acetate of iron for browns, blacks, lilacs, &c.; and acetate of alumina for all the different shades of reds and yellows.

The best method that I am acquainted with of preparing the acetate of iron for making the common black colour for calico-printing is as follows:—Take the iron liquor of its full strength, or lowered with one or two waters according to the goodness of this mordant in the first instance, and then add about 2½ lbs. of flour to every gallon of the fluid, more or less according to the goodness of the flour and the kind of work for which it may be designed. Having well incorporated the flour with the iron liquor, let the whole remain twelve hours or more. After this it is to be boiled for about half an hour, or till the whole be converted into a paste of due consistence, when it will be fit for use. The materials become better incorporated, and the colour is found to work better, by lying thus long before it is finished by boiling.

Formerly the acetate of iron was made by digesting old iron hoops in sour beer, or in very weak vinegar; but of late years it has chiefly been made with the pyroligneous acid,* the oleaginous impurities of which tend, in some cases, to improve the mordant.

Blacks are also produced by the nitrate of iron† and gallic acid; the mixture is called chemical black. The nitrate of iron is made by dissolving metallic iron in a peculiar kind of aqua-fortis. Common aqua-fortis will not answer for this purpose; for, though it may dissolve the iron with rapidity, part of the metal is apt very soon to precipitate; which not only weakens the colour, but leaves the remainder so acidulous that there is always a danger of such a preparation injuring the texture of the cloth.

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* If wood be submitted to an intense heat, when inclosed in an iron vessel of any kind with a proper aperture to allow the vapour to pass, this vapour on being condensed forms the acid in question, and is now known to be a kind of impure *vinegar*. The wood in this case is converted into *charcoal*, of which a great deal is prepared by this process, particularly for the formation of gunpowder.

† Nitrate of iron was not applied to calico-printing till within the last fifty years. This discovery formed an important æra in the trade, as it afforded the manufacturer the means of varying his styles of work in a multiplicity of ways and forms, which till then were entirely unknown.

Chemical black is made by mixing a decoction of galls with the nitrate of iron just described; this produces gallate of iron, and probably some tannate of that metal: the disengaged nitrous acid then dissolves the two newly formed salts. The colour called chemical black may therefore more properly be said to be an union of the colouring matter of galls with the peroxide of iron, than a mixture of gallic acid with nitrate of iron. The gallic acid is probably like the pyroligneous, or nothing but the acetous acid holding a peculiar vegetable substance in solution, and either the muriate or the sulphate of iron, if applied to cloth in conjunction with the colouring matter of galls, will in time produce as intense and durable a black as the nitrate; but the difficulty of thickening the solutions of these salts, and the time which is required to convert their base into a peroxide, render them less fit for the calico-printer's purpose.

It is, however, necessary to remark that the black which is formed by this solution of iron, is produced in a different way from blacks in general; for, when common iron liquor is used for this purpose, it is first printed on the calico; and when it has been sufficiently oxidized by exposure to the air, the goods are boiled in a decoction of madder, which renders such parts as had been printed with the acetate of iron an intense black. But the black from nitrate of iron and galls is applied at once to the cloth, and is not afterwards increased in intensity, or raised, as it is termed, by dyeing.

The calico-printer by using a black ready formed is thus enabled to mix it with other colours, in cases where by dyeing alone it could not be produced, as in conjunction with yellows and olives, raised by weld or quercitron bark.

The acetate of alumina is prepared by a mixture of the sulphate of alumina and potash with acetate of lead, both in a state of solution; so that, on the theory of double decomposition, sulphate of lead is formed, which precipitates, while the acetate of alumina remains in solution.

Since the demand for this article has been increased on account of the extension of the printing-trade, it has been prepared from the pyroligneous acid, by means of lime and alum. The following is the method:

The pyroligneous acid is first passed through a still, to divest it of a portion of the oil or tar which is always dissolved in it; it is then saturated with lime or whiting; and lastly, the acetate of lime thus formed, is decomposed by a heated solution of sulphate of alumina and potash. The result of this double decomposition is sulphate of lime, which precipitates, and acetate of alumina, which is drawn from the sediment of the calcareous sulphate, and preserved for use.

And here it may be necessary to caution the manufacturer against a misfortune that may befall him if he be not conversant with the chemical nature of the substances he employs.

Magnesian limestone abounds in Derbyshire and in some of the adjacent counties; and should a maker of acetate of alumina employ such lime in his process, the article which it would produce would in all probability be entirely unfit for the use of the calico-printer. But I must be more explicit.

In employing the common lime in conjunction with alum, a sulphate of lime will be formed, as mentioned above, and this being nearly an *insoluble* salt, will precipitate. But here, sulphate of magnesia would also be formed, which being a *soluble* salt, would remain in solution, and increase the specific gravity of the liquor, a circumstance which would be very apt to occasion the deception which I am anxious should be avoided. If magnesian limestone be employed, the liquor will appear good by the hydrometer; but as it will contain more Epsom salt than acetate of alumina, it will be unfit for every purpose for which it was intended.

While speaking of acetate of alumina, I cannot avoid remarking, that the process which has just been described for making this mordant, and which is followed invariable by many of the manufacturers in the North, is extremely improper, on account of the lime which is employed in it, be the lime ever so good, as that earth is very prejudicial to every species of red dye. The proper way of making it, though more expensive, is that which was originally pointed out by Berthollet, and which consists in decomposing sulphate of alumina and potash (common alum) by means of saccharum saturni, or acetate of lead.

In reverting to the remaining processes of the print-work, it must be noticed, that when the goods have passed through the weld or madder copper, they are usually carried to a boiler containing wheat-bran and water, in which they are winched for a considerable time, for the purpose of freeing the white grounds from the stain which they had acquired from the madder or the weld. This process always impairs, in some measure, the intensity of the colours; but it is a necessary operation, as there is no other mode so convenient for removing the stain which is always given to the white part of the print by the madder, the bark, or the weld, which has been used in dyeing it.

Branning has also the effect of giving a pink hue to all madder reds. But is not generally known what a peculiar richness may be imparted to madder colours, by raising them with a mixture of bran and madder; that is, by mixing a portion of bran with the madder in the first instance. I have myself sometimes produced colours in this way whose brilliancy has astonished me.

This mode of dyeing red with a mixture of bran and madder is called in the trade *Growse's* or London pink, and was invented by a journeyman calico-printer near London, of the name of Growse, who sold the recipe for 100 guineas. In this process the acid of the bran prevents the precipitation of the *fawn-coloured* particles of the madder, and the red produced is consequently more rosy and pure.

Madder contains two distinct species of colouring particles, a beautiful red or rose colour and a degraded yellow or fawn colour —both are precipitated on the cloth, in the usual dyeing process, to the great injury of the *tint* obtained. The object of the tedious Turkey-red process is to fix the red colouring particles so firmly as to allow the fawn-coloured ones to be dissolved and discharged by long boiling in solution of soap or soda, by oxymuriate of soda or by acid solutions of tin. A dilute supersulphate of potash has lately been used with success for this purpose in France.[*]

It frequently is the case, however, that goods will not bear to be sufficiently branned to clear the whites entirely by that one operation;[†] such goods, therefore, are partially cleansed in the branning-copper, and are then laid on the grass for some days, until they become perfectly clean and white.

But, within the last few years a new method has been introduced, which consists in immersing the pieces for a certain time in a very weak solution of one of the bleaching salts, such as oxymuriate of potash, soda, or magnesia. This simple process, which effects in a few minutes what would require more than as many days in grass-bleaching, is now much practised, and promises very soon to supercede crofting entirely. This is a most important improvement, as some of the large printers formerly required as much land to spread out their goods upon, as would make a farm of a very considerable size.

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[*] See "A Collection of Observations on Madder," by Berthollet, *Annales de Chimie*, tom iv. page 102. M. Watt "On the Properties of Zealand Madder." *Ibid.* page 104. Vogler "On dyeing Cotton with Madder." *Ibid.* page 109.

[†] The temperature at which the operation of branning is performed, is very important. If bark yellows are dyed at 100°, it is customary to bran such goods at 115° or 120°, as it is a principle always to bran at a higher temperature than the goods are dyed at. Madder-work must be branned at a boiling heat.

Besides the kinds of calico-printing already mentioned, there are others which it will be proper to notice in this place. Of these, what is called resist-work, is now done in considerable quantities. It is conducted in the following manner:

A certain preparation of copper, mixed either with flour paste, with gum, or with pipe-clay and gum, is printed on the calico, in any shape or of any pattern that may be desired.* When this is sufficiently dry, the goods are repeatedly dipped in the blue vat till they have acquired that depth of tint which may be required. The art of making an indigo-vat, or blue vat as it is sometimes called, consists in forming such a mixture of lime and sulphate of iron as shall most effectually deoxidize the indigo; as indigo has no affinity for cloth in its natural or oxygenized state. Hence those parts of a piece which are printed with the solution of copper, will never be dyed blue in one of these vats; because the deoxidized indigo becomes oxygenated the moment it touches the copper, which parts with its oxygen to the indigo, and occasions it to become insoluble, and consequently incapable of forming a dye.

Thus, while sulphate of iron has the power of deoxidizing indigo, sulphate of copper, or any other salt of that metal, is incapable of retaining its oxygen, whenever it comes in contact with that singular substance in a state of deoxidizement; and it is a curious instance of the different degree of intensity by which oxygen is held by the different metals.

I suspect, however, there is often great waste in the management of these vats, and that the contents are often thrown away long before the indigo is spent, in consequence of the want of a due admixture of the lime and the sulphate of iron with the colouring matter; whereas, if a dyer or calico-printer be acquainted with chemical science, he will understand the nature of the operation of these substances upon each other, and hence can be at no loss how to keep up the vat to its original strength, by replenishing it occasionally with one or other of the ingredients as may be required.

The fact is, that indigo, unless it be deoxidized, is insoluble either in the alkalies or the alkaline earths; but when this is effected by the iron of the copperas, the lime dissolves it, and forms it into a perfect solution of indigo. At first a portion of the lime abstracts the sulphuric acid from the copperas, and precipitates with it in the state of sulphate of lime. The detached metal being now in the state of the protoxide of iron, and having in that stage of its oxidizement a great affinity for oxygen, it takes it from the indigo, and thus becomes changed to the peroxide of iron, which being insoluble precipitates likewise. Thus the acid and the base of the sulphate of iron are at length both precipitated, when the remaining portion of lime seizes the deoxidized indigo and forms with it the solution desired. When the principle of the blue-vat is thus properly understood, it is an easy thing for the manager, at any time to discover which of the three materials is deficient, and to add it accordingly. I am the more desirous of offering these remarks, because I have many times heard the proprietors of a manufactory declare that they have occasionally thrown away the whole contents of a new vat, containing many pounds worth of indigo, because the foreman or managers of the the works had not observed the due proportions of the lime and the copperas to put the indigo into a proper state for working; and they have an idea, if it is once wrong it must be always so, and therefore they throw it away without attempting to correct the error.

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* The sulphate, the nitrate, the muriate, and the acetate of copper have all been employed for preparing the resist-paste; but I believe the sulphate is the best for the purpose, unless a very concentrated solution of the four salts were prepared by successively dissolving each of them in pure water.

To revert to the process of resist-work, it is necessary to say, that when the pieces have been sufficiently immersed in the blue-vat and have been washed and passed through diluted sulphuric acid, those parts which had been printed with the preparation of copper, are found to be preserved a good white; the preparation having effectually resisted the operation of the indigo, though all the other parts of the cloth have received a permanent dye. The various deep blue calicoes with white spots or white figures, which are now so common, are generally done in this way; and by a similar management with subsequent dyeing in madder, in weld or quercitron bark, figures in red or yellow (as the case may be) are exhibited upon a blue ground.

In some particular styles of work the operation of certain colours is resisted by means of stopping out with wax; but this is too expensive a method to be adopted often, in these times, when it is the object of every manufacturer to finish his prints at the least possible expense. Formerly this mode was very generally practised, and wax was consumed in very large quantities by this process.*

In printing those silk handkerchiefs called Bandanas, a process called waxing is still followed. It consists in making a preparation of tallow and rosin very liquid by heat, and in printing it in that state with a block upon the silk. When such goods are passed through the blue-vat, those parts which are covered with the tallow and rosin are preserved from the action of the indigo, and remain white, while all the rest is dyed a fast blue. The method afterwards taken to discharge a part of this blue, and produce yellow, orange, *&c.* will be mentioned hereafter.

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* In the East Indies, wax is still used for preserving the whites in calico-printing.

A very singular looking substance was discovered a few years ago near Stockport, which being handed about from one to another, created considerable interest in that neighbourhood. Every body supposing it to be a natural production, specimens of it were sent to a variety of persons in various parts of the kingdom, for their opinion and analysis, and among others a portion was sent to myself. However, after every one had been busily engaged in examination and conjecture, respecting this unknown substance, it was announced, that some seventy or eighty years before, a calico-work had stood on the spot where the article was found, and that this was nothing more than a large heap of the refuse compound of flour, wax, and gum, above mentioned.

The reader will perceive that these *resists* are employed for the purpose of preserving certain parts of a piece white, and of giving other varieties to those goods in which blue is the predominant colour; but if the ground is to be white, and the piece is only to have one small object* in indigo blue, such as a single sprig, then a different management is necessary, and the colour is imparted by a process which is called *pencil-blue.*

Here the indigo is deoxidized by means of orpiment, which is a sulphuret of arsenic; and formerly, whatever objects were done with it were put in by means of a pencil: hence its name, PENCIL-BLUE.

Pencil-blue is composed of the following ingredients, viz. Ten ounces of indigo finely ground in water; twenty ounces of quick-lime in lumps; the same quantity of potash of commerce, or the impure sub-carbonate of this alkali; and ten ounces of orpiment. These proportions require one gallon of water, and when the whole has been properly mixed, and time has been allowed for the sediment to subside, the clear colour is to be carefully poured off and thickened with gum senegal.

The intention in using this preparation had always been to impart it to the calico, before the indigo had time to recover its oxygen: hence a small quantity only was mixed at once, or no more than could be pencilled on in a very short time; but the inconvenience which attended this plan, gave rise at length to an invention which has nearly superseded the use of the pencil.

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* *Object* is a technical term belonging to this branch of manufacture.

The invention to which I refer is the spring-sieve, which may be thus described : A fine canvass sieve is suspended within a tub of pencil-blue colour, so contrived that the canvass should be in contact with the colour, and follow it as it is wasted. But as the colour was consumed the sieve did not always exactly sink with it, which occasioned great uncertainty, and sometimes considerable disappointment from the admission of too much atmospheric air :—to obviate these inconveniences many schemes were resorted to, but none of them completely answered the purpose.

This utensil has, however, now been much improved by a particular friend of mine. The improvement consists, essentially, in the addition of a vessel upon the principle of the common bird fountain, which is filled with the prepared indigo and closely corked. This supplies the blue-tub just as fast as it is exhausted.

[*To be continued.*]

ART. XXVII. *On the Barometer*. By J. F. DANIELL, F.R.S. [*Jour. Roy. Inst.*]

THE following paper requires a few words of preface. It contains, as far as I am able to recollect, the substance of a communication which I had the honour to make to the Royal Society, in the month of November last, before the commencement of their last session. As, with a former paper upon the same subject, a mistake had arisen, as I was informed, from my not having expressed a desire to have it read, the consequence of which was that it was not read, I took the precaution of placing the present manuscript in the hands of the President, with a due notification that it was presented for the purpose of being read at the meeting of the society. I went, immediately afterwards, upon the continent; where I was detained several months. Upon my return, in June, I found that the first part of the *Philosophical Transactions* for the year, had been published, and that it contained no notice of my paper : neither could I find that any communication had been sent to me of its destination. I was naturally anxious to obtain some intelligence respecting it, and addressed a note upon the subject to the President; to which he returned me the following reply :

Dear Sir, 26, *Park-street, June* 5.

Your paper was read to the Royal Society many months ago,* and has been before the council. There was but one opinion expressed as to the ingenuity of the method of preventing the introduction of air, supposing the *cause* that which you have assigned. The *existence* of this cause for the appearance of elastic matter in barometer tubes, was not considered, however, as proved by the experiments you have detailed, and it was therefore agreed by the council to wait for your return, in hopes that you might be able to give them some new details or elucidations from the researches which you stated were in progress.

I am, dear Sir,
Very sincerely your's,
H. Davy.

I immediately returned the following answer, being anxious to publish the paper in the ensuing number of this Journal; but not acquiescing sufficiently in the reasons assigned by Sir H. Davy to withdraw the paper from the judgment to which I had submitted it.

My dear Sir,

I have no new details or elucidations to communicate to the Royal Society. I shall therefore be much obliged to you to have my paper brought again before the council for their final determination; unlesss indeed I am to understand that their determination has been already expressed.

I have the honour to be, dear Sir,
Very truly your's,
J. F. Daniell.

To Sir H. Davy, Bart.,
President of the Royal Society, &c.

In consequence of this, the paper was again laid before the council, just before the recess; when, on account of a division of opinion, their determination was postponed to next year. A member of the council then asked leave to withdraw the paper, in order to allow of its publication elsewhere, and stated that I had neglected to preserve a copy of the manuscript. The president, I am told, objected to this, and laid down the law, that the paper having been once taken into consideration by the council, could not be withdrawn. To make sure that no formality was neglected, I immediately addressed the following note to the president:

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* It was read, I am informed, on the twentieth January, and several papers which were delivered in after its communication were read before it.

My dear Sir, *Gower-street, 25th June,* 1825.

Understanding that the decision upon my paper is postponed to next year, I beg permission to withdraw it.

I remain, dear Sir,
Your's faithfully,
To Sir H. Davy, Bart., J. F. Daniell.
President of the Royal Society, &c.

To this application I never received any answer. This is a brief statement of facts, upon which I shall abstain from making any comment. Twice before I have experienced similar treatment from the council of the Royal Society, and twice before I have appealed to the scientific public with success. I shall now endeavour, from memory and such rough notes as I have preserved, to recompose the paper, with a full conviction that the facts narrated are of sufficient importance to call for immediate publication, and satisfied by the opinion of those who are well qualified to judge, that there is sufficient proof of their existence to satisfy all impartial minds that the subject is well worthy of that further investigation which it was my purpose to propose to the Royal Society. A new law has now been promulgated by the President of the society, and every thing henceforward published in the *Philosophical Transactions* must be considered as proved. The council will doubtless immediately drop the notice which has always hitherto been published in the preface to the volumes, that "they do not pretent to answer for the certainty of the facts or propriety of the reasonings contained in the several papers published, which must still rest upon the credit or judgment of their respective authors;" and they will, of course, be careful that this law is administered with impartiality. In such a regulation I, as an humble individual, cannot but acquiesce; but, with many others I imagine, I shall always be found to resist the curtailment of the hitherto-acknowledged right of an author to withdraw a paper, any time previous to the decision of the council upon its publication.

In the year 1823 I presented to the Royal Socicty a paper upon the Construction of the Barometer, the substance of which I afterwards published in my volume of Essays, the original paper having been committed to the archives of that learned body. I therein stated my reasons for differing from the high authority of the President, upon the *cause of the existence of elastic matter in borometer tubes*, suggested to him in a paper upon "the electrical phenomena exhibited in vacuo," and published in the *Philosophical Transactions* for the year 1822. Sign. Bellani also arrived at the same conclusion as myself, from a series of experiments which he undertook, expressly to determine whether the air or vapour, the last portions of which are found to remain so obstinately in barometers and thermometers, is introduced with the mercury, or is a portion of that which originally occupied the tube before the introduction of the metal. The conclusion he comes to is, that it is always a portion of that which previously adhered to the glass, and *that mercury is utterly incapable of absorbing either air or moisture.* One of his experements is so simple, and at the same time so conclusive, that I cannot refrain from giving a short account of it. He filled a barometer tube, and boiled it very carefully, and then prepared a funnel made of a small capillary tube, which reached through the mercury in the barometer-tube, to the closed end, and was enlarged at the top. When introduced, it had been recently made, and perfectly dry. Some mercury was then prepared by agitating it in a bottle with water and air, and dryed by means of filtering paper, and afterwards passing it through paper cones, three or four times, into dry vessels. A little of this mercury was poured into the funnel tube, and the air extracted by means of a fine wire, so that the column was continuous. So much of this prepared mercury was then poured in as fully to displace the mercury which had been boiled in the tube. The barometer was found to stand exactly at the same height as before in the same circumstances; and, when the mercury was heated, none of those bubbles appeared which arose on the first boiling.*

Still further to illustrate this subject, which I thought of the highest importance, and to ascertain the difference of capillary action in boiled and unboiled barometer-tubes, I undertook the following experiments. The apparatus, which I made use of, consisted of an upright pillar of brass, standing upon a mahogany foot, upon which two horizontal arms of unequal lengths were made to slide; at the extremity of each of these a steel needle, with a fine point, was fixed perpendicularly downwards. These points could be adjusted to the same plane, or their relative distance be measured, by means of a nut and screw upon the pillar, which carried a *nonius;* and the slightest contact of these points with the clean surface of a basin of mercury was instantly perceptible. I satisfind myself, by repeated trials, that the adjustment might be depended upon to the one-thousandth part of an inch. I made a contrivance to hold a glass tube perpendicularly immersed in a basin filled with mercury; and when one of the steel points was made to touch the surface of the fluid in the tube, and the other the surface in the basin, the depression of the former was accurately measured by the *nonius*. In this manner I determined the capillary action of several tubes, varying in their diameters from one to six tenths of an inch. The results agreed as nearly as possible with Dr Young's table, calculated from the experiments of Mr Cavendish. The end of the tubes, opposite to those at which the measures had been taken, were then hermetically sealed, in such a manner as to be readily re-opened under mercury: they were immediately filled with mercury, and carefully boiled. I expected to be able to ascertain the differences of depression by opening them in the basin of mercury, and proceeding as before. The experiment was performed as soon after the operation of boiling, as the mercury in the tube had cooled down to the temperature of that in the basin. At first the attraction between the mercury and the glass appeared to be perfect, and no depression could be perceived. When, however, the tubes were left some time exposed, either before or after they were opened, the air and moisture insinuated themselves between the metal and the glass, and an immediate depression was the consequence. This depression increased gradually, till at length it became fixed at the exact point of that of the unboiled tube. The progress of this effect was easily perceptible with a magnifying glass, and was rendered still more visible by heating the tube, when air-bubbles were immediately detached. This is obviously the same effect as that described by Sir H. Davy in his paper before alluded to, in which he says, that, "on keeping the stop-cock of one of the tubes, used in the experiment on the mercurial vacuum, open for some hours, it was found that the lower stratum of mercury had imbibed air, for when heated in *vacuo* it emitted it distinctly from a space of a quarter of an inch of the column; smaller quantities were disengaged from the next part of the column, and its production ceased at about an inch high in the tube." Now, I believe that I should not be too presumptuous if, stopping here, I were to maintain that my experiment presented absolute *proof* that the air had insinuated itself between the mercury and the tube, and shewed that there was no "reason to believe that this air existed in mercury in the same invisible state as in water, that is, distributed through its pores." For, if the latter had been the case, the mercury, which contained no air after being boiled, would, from its greater density, have sunk in the tube, when surrounded by mercury which had not been boiled, and would have risen gradually as it became saturated with air. I am justified in drawing the conclusion from the contrary effect, that the air had insinuated itself between the metal and the tube, for the capillary depression is known to be in inverse proportion to the affinity of the fluid for the containing tube, and nothing could have affected the affinity in the case before us, but the gradual interposition of a thin stratum of air and moisture.

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* *Giornale de Fiscica*, Vol. vi. p. 20, or see *Royal Institution Journal*, Vol. xv. p. 371.

Having thus traced and measured the progress of the air down the sides of small tubes filled with mercury, and boiled with the greatest care, I was naturally led to suspect that the same action might take place in barometers, to their gradual deterioration. I soon saw reason to conclude that such a process actually was going on in the most carefully constructed instruments, and that, in time, air would thus insinuate itself into the best Torricellian vacuum. In the paper upon the construction of the barometer, to which I have before alluded, I gave all the particulars of the making of two barometers, in which every precaution was used to dispel every particle of air. One of these was of very large dimensions, and was fixed up in the apartments of the Royal Society, under the superintendence of the Meteorological Committee. The other was of the mountain construction, and intended for my own use. The agreement between these two instruments, when all corrections were made for the differences in their sizes and forms, was very perfect, and proved that the care which had been bestowed upon them had not been thrown away. In the latter, however, I lately remarked that a small quantity of air had ascended into the *vacuum*. I could not discover any way in which this could have obtained admission; but, attributing it to accident, I laid it aside and thought no more of it till the present experiments recalled it to my recollection. By a singular coincidence I was, about this time, informed that the barometer of the Royal Society had assumed a very remarkable appearance, and that the mercurial column, which was originally perfectly bright and compact, now seemed dull and speckled. I immediately proceeded to examine it carefully, and I at once perceived that it was copiously studded with minute air-bubbles. As far as the mercury was exposed to view, the specks could be traced decreasing in size, from the upper to the lower part. The manner in which this instrument is fixed rendered it impossible to suspect that this air could have obtained admission by any accident; for, unlike the mountain barometer, the column of metal is exposed to no oscillations but such as arise from differences of atmospheric pressure. I was myself quite satisfied, and those who have read the account of the precautions taken in filling the tube will also, I think, be satisfied, that this air was not left at its original construction. I now leave it to the candid to judge, whether "the cause which I have assigned for the appearance of this elastic matter in barometer tubes has not *already* been proved by the experiments which I have detailed."

While I was thus occupied with these considerations, and sufficiently vexed to find that all my care had been thrown away to prevent my adopting that opinion without very strong grounds, it occurred to me that I had, in the course of my experience, observed a phenomenon, which was calculated to throw some light upon the present question; namely, that gasses were more readily preserved from mixture with atmospheric air over water than over mercury. I was unable to refer to any notes of experiments to confirm this suspicion, but I proposed the question to Mr Faraday, who, I made no doubt, from his great accuracy and experience, must have made the observation, if it were founded in fact. Without at the time having any knowledge of the ulterior object which I had in view, he at once answered me, that mercury, he believed, would not confine gases for a long period so well as water; and he thought, that by referring to his note-book, he could furnish me with the particulars of a case in point. He accordingly did me the favour to extract the following particulars:—

In June, 1823, he made a mixture of one volume of oxygen and two volumes of hydrogen; with this he filled five *dry* bottles over mercury, and also four bottles over water. He left the glasses inverted over mercury and water, placing three mercury and two water bottles in the windows, so as to receive the sun's rays and day-light; and two mercury and two water bottles he placed in a dark place. In July, 1824, he examined the bottles; the water bottle in the light contained hydrogen and some common air, and there was no alteration of volume; the mercury bottle in light contained common air only. The water bottles in the dark place showed no alteration of volume, and the air contained in them proved to be the *original mixture;* the mercury bottles in the same situation contained *nothing but common air.*

Now, if I had not known, from the authority of the President, that the council of the Royal Society did not think this *sufficient proof*, I should very confidently have concluded from these facts, that a fluid which has attraction enough for glass, to enable it to wet its surface, effectually prevents the passage of gasses into or out of vessels of that substance; while a fluid which does not wet the surface permits their slow penetration. I should, moreover, have ventured to affirm, that the case of the confined air is exactly analogous to that of the barometer; for its escape and the admission of the atmospheric air can only be in virtue of the law discovered by Mr Dalton, that the gases are as *vacuo* to one another. The inference is also pretty strong, that the filtration takes place along the surface of the glass, and not through the pores of the fluid.

It has been attempted, I understand, to controvert this conclusion, by the observation that gases have been preserved a considerable time by mercury; but when it is considered that the slight film of moisture, or any foulness of the mercury, will form a connexion between the metal and the glass, the objection can be of no avail, unless these circumstances have been attended to. To ensure the maximum of the effect which I have been describing, it is necessary that both glass and mercury should be in the driest and cleanest possible state; that is to say, exactly in the state in which they exist in a well made barometer.

That another attempt has been made at explanation I can scarcely credit; namely, that some facetious person had played Mr Faraday a trick. The particulars of the case disprove the possibility of such a circumstance, unless upon the supposition that such person foresaw the present discussion. The character, however, of Mr Faraday for precision, renders it unnecessary to say any more, than that he informs me there is not the slightest ground for such a suspicion.

I was no sooner convinced that the most carefully constructed barometers were liable to a slow and gradual deterioration in the manner which I have indicated, than I endeavoured to find a remedy to the evil; without which, it is clear, that some of the most interesting problems of meteorology must be for ever left in a state of vagueness and uncertainty. For a long time I despaired of success; but when at length I discovered an effectual method of preserving the Torricellian *vacuum*, I flattered myself that the Royal Society would so far have taken an interest in the subject as to have ordered it to be submitted to a trial, which could not have been so satisfactorily made under any other superintendence.

I soon perceived that the only possibility of effecting the object which I had in view, consisted in discovering some method of making the mercury *wet* (if I may be allowed the term) the tube in which it is contained. I was fearful, at first, that all the substances, to which its attraction is sufficiently strong for this purpose, would be so much acted upon as to become disintegrated or dissolved. I, however, fortunately recollected that, in some experiments in which I was formerly engaged, platinum, immersed in boiling mercury, became completely coated by it, and afterwards retained its coating for a long time. I repeated the experiment with some platinum foil, and found it to succeed perfectly. The mercury adhered strongly to the foil, and the latter, after a long immersion, was found to have lost none of its tenacity. I availed myself of this property in the following way:—I caused a small thin piece of platinum tube to be made about the third of an inch in length, and of the diameter of the glass tube; this was carefully welded to its open end, so that the barometer tube terminated in a ring of platinum. The tube was filled and boiled as usual, and the infiltration of air was completely prevented by the adhesion of the mercury, both to the interior and exterior surface of the platinum guard. I have no doubt that a mere ring of wire welded, or even cemented upon the exterior surface of the glass, which would be a much easier and less expensive operation, would be a sufficient protection, as the slightest line of perfect contact must effectually arrest the passage of the air: but in the first attempt I was desirous that the experiment should be tried in the most perfect manner. When a piece of glass, armed either with a ring or tube, is immersed in mercury, the effect is easily perceived; instead of any depression being visible around it, the mercury may be lifted by it considerably above its proper level. Time, of course, will be requisite fully to confirm the efficacy of the guard, and I was in hopes that the Royal Society would have attributed weight enough to the observations which I submitted to them, to induce them to give orders for the construction of a large barometer, upon the principle which I have suggested, to be placed beside the two others already in their possession. An opportunity would thus have been afforded of determining, in the most unexceptionable manner, several facts of the utmost importance to meteorological science. The further experiments which the council have called for, before they would commit themselves by the publication of the foregoing opinions, are in progress, and shall be laid before the society as soon as they are complete; but they obviously require considerable time for their progress. I have taken the pains to re-write this paper, under the conviction that advantage will be derived to science, by sooner throwing the subject open to general observation and experiment.

I expected to have been able to find some evidence of the deterioration of barometers in the numerous registers that are kept of their oscillations: but I have not discovered any which have been continued for a sufficient length of time, with the same instruments, to answer this purpose satisfactorily. Instances abound of observers having taken the pains to re-boil their barometers, from air having obtained admission, in some unknown way which has always been attributed to accident; but the fact of their gradual deterioration cannot, in this way, be established so completely as might have been supposed..

The register of the Royal Observatory at Paris has only been published since the year 1816, in the *Annales de Chimie*; a period which is not sufficient so far to neutralize the annual oscillation as to afford the means of a satisfactory comparison. Mr Howard, however, in his work upon the climate of London, states the mean height of the Royal Society's barometer for ten years, from 1797 to 1806, to be 29.882 inches, while for the ten succeeding years, it is only 29.849 inches, which give a depression of .033 in that interval.

The observations of the following ten years will not, I fear, be available in the same comparison, from the carelessness with which they have been made.

The difference in the height of the old and new barometer, which have now been placed side by side, was, in the latter part of the year 1824, .07 inch, upon a mean of twenty observations; the new barometer standing upon the average so much higher than the old one. Whether this be wholly owing to detorioration, it is not possible to say; for the old barometer does not appear to have been boiled: but from the well known accuracy of Mr Cavendish, under whose superintendence it was constructed, it is impossible not to ascribe a great portion of it to this cause. The mercury of this instrument is now thickly studded with air-bubbles of much larger size than those of the new barometer: and when I last examined it, some of them were just upon the point of making their escape.

[*To be continued.*]

Art. XXVIII. *Mr Children's Summary View of the Atomic Theory according to the Hypothesis adopted by M. Berzelius. [Ann. Phil.]*

[*Continued from page* 193.]

The weight of the atom of any body is easily determined, if we know correctly the composition of one or more of the combinations it is capable of forming with any other body, the weight of whose atom has been previously ascertained. Sulphur, for instance, combines with oxygen in several proportions; in the lowest, 100 parts of sulphur take 50 of oxygen; in the next, 100; and the third, 150;* numbers which are in the ratio of

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* There is a fourth compound formed of an atom of sulphurous acid united to an atom of sulphuric acid, and containing 100 sulphur + 125 oxygen. Its atomic composition may be stated as just mentioned, or, as consisting of 2 atoms sulphur + 5 atoms oxygen. It is not necessary to say more about it in this place.

1, 2, 3; we may, therefore, assume that in the different oxydes an atom of sulphur is united successively to 1, 2, and 3 atoms of oxygen, and the supposition is supported by various considerations of the other combinations of sulphur, as, for instance, those of the sulphurous and sulphuric acids. The lowest compound, therefore, may be considered as containing an atom of each element, and if we call that of oxygen 8, we find by a simple proportion that that of the atom of sulphur is 16.[*]

This example is sufficient to show the method to be adopted in similar researches, and it is evident that when the weight of the atom of any one body is ascertained, it may be employed for determining that of other bodies.

The results of a mineral analysis may be calculated on the atomic theory, and the inevitable small errors of experiment corrected by its means.

Suppose we have found that a sulphuret of lead is composed of

Lead	86
Sulphur	14
	100

Here a certain number of atoms of lead, whose total weight is 86, were combined with a certain number of atoms of sulphur, whose weight is 14. If, therefore, we divide 86 by the number representing the weight of the atom of lead (which we find in the tables is 104), and 14 by that of the atom of sulphur, (16), suppressing the decimal point in both cases, we find that the compound contains 82 atoms of lead and 87 atoms of sulphur, numbers which are very nearly equal. Hence we conclude that the mineral is composed of 1 atom of lead and 1 atom of sulphur; and if we calculate the results which our analysis ought to give on this supposition, we find the numbers to be

Lead	86·66
Sulphur	13·33

which accord very nearly with the results of the experiment.

A similar operation will enable us to find the atomic composition of all other binary compounds, whose analysis is known.

Let us now take an instance of some more complex compounds, and calculate them on the data and numbers assumed by Berzelius.[†]

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[*] I adopt the numbers given by Brande and Phillips, in which hydrogen is taken as unity.

[†] In which oxygen = 100. The examples are taken from Beudant, p. 225, et seq.

Suppose an analysis of molybdate of lead (a ternary combination) had given

Oxide of Lead	61
Molybdic acid	39
	100

We find in the annexed table, that the quantity of oxygen in oxide of lead is 7·171 per cent. and that in molybdic acid 33·45; consequently 61 of the former contain 4·37 of oxygen, and 39 of the latter 13·04; but 4·37 : 13·04 :: 1 : 3; or the oxygen of the acid is three times that of the base; but we observe in the tables that the base contains only 2 atoms of oxygen, whilst the acid contains 3; therefore to preserve the ratio of 1 : 3, there must be 2 atoms of acid to 1 of base. The results of the analysis calculated on these data give

Oxide of lead	60·86
Molybdic acid	39.14
	100·00

Let us next take an analysis of copper pyrites, and suppose that it has given us

Copper	34
Iron	30
Sulphur	36
	100

The atom of copper by the table is 791·39; that of iron 678·43. Therefore $\frac{34}{791·39} = 429$ atoms of copper; $\frac{30}{678·43} =$ 442 atoms of iron, and $\frac{36}{201·16} = 1789$ atoms of sulphur. Now these numbers are nearly as 1, 1 and 4, and consequently the sulphur must be equally divided between the two metals, so as to form bisulphurets, each containing 1 atom of metal, and 2 atoms of sulphur. If we calculate the composition of the pyrites according to these numbers, we shall have

Bisulphuret of copper	52·48
Bisulphuret of iron	47·52
	100.00

Or if we take the elements separately,

Copper	34·79
Iron	29·82
Sulphur	35·39
	100·00

which agrees very nearly with the experimental results, and confirms their accuracy.

Let us now take the analysis of a quaternary compound, a variety of emerald, which gave

			Atoms.
Silica	68·64	or oxygen 34·52	= 8
Alumina	17·96	8·38	= 2
Glucina	13·40	4·17	= 1
	100·00		

By the tables, we find the respective quantities of oxygen in the three elements of the mineral as stated above. Now we may consider this compound (says M. Beudant) in two ways, either as consisting of one base (glucina) united to a double acid (silica and alumina), or as a double salt formed of the silicate of alumina and silicate of glucina; both views lead to the same conclusion. In the first case the mineral is supposed to consist of 2 atoms of acid (composed of 4 atoms of silica and 1 atom of alumina) combined with 1 atom of glucina. In the second manner of considering the compounds, the general law proposed by Berzelius requires that the acid of one of the salts should be a multiple by a whole number of the acid of the other, which may happen in different ways, but in consequence of the tendency of glucina to form salts with excess of acid, the most simple mode is to consider the silica as equally divided between the two bases, which gives us a quadrisilicate of glucina and a bisilicate of alumina. The first of these salts contains 4 atoms of silica and 1 atom of glucina, forming 1 atom of quadrisilicate; the second contains 4 atoms of silica, and 2 atoms of alumina, forming 2 atoms of bisilicate, because all the oxides contain the same number of atoms of oxygen. The composition of the mineral on the first supposition is,

Quadrisilicate of alumina	86·28
Glucina	13·72
	100·00

And on the second,

Quadrisilicate of glucina	47·71
Bisilicate of alumina	52·29
	100·00

which are composed of

Silica	33·99
Glucina	13·72
Silica	33·99
Alumina	18·30
	100·00

Or,

Silica	67·98
Glucina	13·72
Alumina	18·30
	100·00

Let us take another example of a quaternary compound as a good specimen of the mode of reasoning adopted in these calculations.

The analysis of zoisite gives

		Ox.	Atoms.
Alumina	33	= 15·42	= 2
Silica	43	= 21·62	= 3
Lime	24	= 6·74	= 1
	100		

In this case the silica must be so divided between the two bases as to form a silicate of alumina containing 2 atoms of silica and 2 atoms of alumina, and a silicate of lime, in which the quantity of the oxygen in the acid is equal to that in the oxide; the oxygen in the first salt is, therefore, double the oxygen in the second. Now lime contains 2 atoms of oxygen, and silica 3 atoms; consequently to preserve the equality of oxygen in the two bodies, there must be 3 atoms of lime and 2 atoms of silica. The total quantity of oxygen in this silicate is, therefore, 12, and that in the silicate of alumina 24; but in that compound there are only 6 atoms of oxygen; therefore the salt must contain 4 atoms of silicate of alumina. According to this, we have

Silicate of alumina	59·47
Silicate of lime	40·53
	100·00

which are composed as follows:

Alumina	30·66
Silica	28·81
Lime	26·13
Silica	14·40

Or,

Alumina	30·66
Lime	26·13
Silica	43·21

Hence if the substance operated on was pure, a small portion of lime has, in the analysis, been confounded with the alumina.

As another example, and one well worthy to follow the preceding, we will take the analysis of a variety of topaz. The results gave

		Ox.	Atoms.
Alumina	59	= 27·55	= 5
Silica	34	= 17·10	= 3
Fluoric acid	7	= 5·09	= 1
	100		

We may consider this mineral either as a compound formed by the combination of a double acid (silica and fluoric acid) with alumina; or as a double salt, consisting of one base united to two different acids; that is, as a fluate, and a silicate of alumina. In this instance, the alumina naturally divides itself into two portions, whose quantities of oxygen are 3 and 2. The first portion is combined with a quantity of silica, containing 3 atoms of oxygen, and forms a silicate; the second is combined with a quantity of fluoric acid, whose oxygen is 1. Hence it follows that the oxygen of the first salt is to the oxygen of the second in the ratio of 6 : 3, or 2 : 1. Now fluoric acid contains 2 atoms of oxygen, and alumina 3; the bi-aluminous fluate must, therefore, be formed of 4 atoms of alumina and 3 atoms of acid, in order to preserve the ratio of 2 : 1; the oxygen of this salt, therefore, is 18; but an atom of silicate of alumina contains only 6 atoms of oxygen, because these two oxides have each 3 atoms; and as the oxygen of the silicate must be double that of the fluate, there must be in this compound 6 atoms of silicate. On these data topaz is formed of

Silicate of alumina	68·70
Bi-aluminous fluate	31·30
	100·00

Or, taking the elements separately,

Silica	33·00
Alumina	35·72
Fluoric acid	7·61
Alumina	23·67
	100·00

Or,

Silica	33·00
Fluoric acid	7·61
Alumina	59·39
	100·00

Or, on the first hypothesis,

Fluo-silicic acid	40·61
Alumina	59·39
	100·00

In his *Nouveau Système Minèralogique*, Berzelius frequently calculates the results of the analyses of minerals consisting of metallic alloys, or sulphurets, from the quantity of oxygen which each ingredient would take if reduced to proportionate degrees of oxidation.

An ore of antimoniated silver, analyzed by Klaproth, gave

Silver	77
Antimony	23
	100

and its atomic constitution is thus calculated by Berzelius,*

Argent	77	prenant oxygene in degrés	5.798.	2.	77
Antimoine	23	proportionels d'oxidation.	2.850.	1.	23

Now the weights of the atoms of silver and antimony in Berzelius's table are,

Silver = 2703
Antimony = 1613

and the protoxide of silver contains 2 atoms of oxygen, and that of antimony 3, and no lower states of oxidation of either metal are mentioned:

2703 : 200 :: 77 : 5·69
1613 : 300 :: 23 : 4·27

But 5·69 : 4·27 is not in the ratio of 2 : 1; to obtain which, and reduce the metals to a *proportionate* degree of oxidation, recourse is had to an imaginary oxide of antimony, at a lower degree of oxidation than the lowest known oxide of that metal; and it is consequently assumed that the 23 parts of antimony, if reduced to the state of oxide, would require only two-thirds as much oxygen as by the tables they ought to take. Is this good logic?

We have now to exhibit the method invented by the same philosopher for denoting the composition of chemical compounds by symbols, "in order to facilitate the expression of the proportions of their elements, and to enable us to state briefly and easily the number of elementary atoms which any of them may contain." The nomenclature is wholly taken from the Latin.

1. Simple bodies not metallic are denoted merely by the initial letter of the Latin name of each substance, even though the same letter be common to some of the metals; thus S = sulphur; C = carbonicum, P = phosphorus, B = boracium, (boron), &c.

2. A metal whose initial letter is not common to any other elementary body is denoted, like the preceding substances, by

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* Nouveau Système, p. 50.

the letter alone, as U = uranium, K = kalium (potassium), L = lithium, &c.; but if the initial be common to another metal, or to either of the simple non-metallic substances, then the two first letters are taken, as Si = silicum, Au = aurum, &c.; but if both of the first and second letters be common to more than one metal, then, instead of the *second* letter of the name, the *first different consonant* is annexed to the initial letter. Thus St = stibium, Sn = stannum, &c.

The chemical sign only denotes a single atom; if it be necessary to express more than one atom, a figure is placed to the left of the sign; thus, Cu + O, denotes oxidulous copper, and Cu + 2 O, oxide of copper; but as this method would be inconveniently long for expressing the composition of a compound of the second order, Berzelius abridges it by omitting the letter O, and denoting the atoms of oxygen by dots placed over the sign of the base, the number of dots indicating that of the atoms of oxygen combined with it. The oxides of copper, for instance, instead of the preceding signs, are indicated by $\dot{C}u$, and $\ddot{C}u$; the sign of sulphurous acid is $\ddot{S}$, that of sulphuric acid $\dddot{S}$, and so on; and in the salts of copper $\dot{C}u\,\dddot{S}$, = oxidulous sulphate of copper, and $\ddot{C}u\,\dddot{S}^2$, sulphate of copper; the little figure placed above, like an algebraical exponent, indicating that in the latter compound there are 2 atoms of sulphur or sulphuric acid to 1 of base.

The composition of atoms of the third order is denoted after the same manner; for instance, $\ddot{C}a\,\ddot{C}^2 + \ddot{M}g\,\ddot{C}^2$ represents the mineral called *dolomite*, which is composed of an atom of carbonate of lime, and an atom of carbonate of magnesia. The formula for alum is $\ddot{K}\,\dddot{S}^2 + 2\,\dddot{A}L\,\dddot{S}^3 + 48\,Aq$; and indicates its composition to be 1 atom of sulphate of potash + 2 atoms of sulphate of alumina + 48 atoms of water (aqua). The small exponential figure refers only to the initial sign immediately preceding it; but the coefficient applies to each element contained between the sign +; as, for instance, in the preceding examples, the exponent ^{3} means that 3 atoms of sulphuric acid combine with the atom of alumina to form the sulphate, and the coefficient denotes that 2 atoms of that salt are taken.

So far the symbols are tolerably simple and intelligible; but we frequently meet with such expressions as the following:— $\dddot{A}l\,\dddot{S}i$, silicias aluminicus; $\ddot{A}l^2\,\dddot{S}i$, silicias bialuminicus; $\ddot{C}a^3\,\dddot{S}i^2$, silicias calcicus; $\ddot{C}a^3\,\dddot{S}i^4$, bisilicias calcicus; $\dddot{A}l\,\dddot{S}^3$, sulphas aluminicus; $\dddot{Al}\,\dddot{S}$, sulphas trialuminicus; $\ddot{Ca}\,\dddot{S}^2$, sulphas calcicus, &c. which require a little further explanation.

In order to understand the meaning of these and similar formulæ, it is necessary to state some peculiarities in Berzelius's views with respect to the composition of certain bases.

If we refer to Thomson's or Phillips's table of the weights of atoms, we shall find that lime, baryta, strontita, alumina, magnesia, &c. as well as most of the protoxides of the common metals, as lead, iron, tin, mercury, &c. &c. contain respectively 1 atom of base and 1 atom of oxygen. But Berzelius considers them as containing 2, and some of them 3 atoms of oxygen, for the following reason:—"If we take," he observes, "a comprehensive, general view of the compound bodies that have been correctly analyzed, we find that many of them, particularly the oxides, contain decidedly more than 2 atoms, and that it most frequently is the electro-negative element which enters in a greater proportion than that of a single atom; soda, oxide of lead, carbonic acid, sulphuric acid, &c. may be quoted as familiar instances. This is still more observable in the combinations of compound atoms, as in the salts, where several atoms of the electro-negative oxide are commonly found united to a single atom of the electro-positive. On the other hand there is every reason to believe that the atoms are only united one to one in those bodies which manifest weak affinities, as the gaseous oxide of carbon, the oxidules of copper, mercury, gold, &c.; so that we may presume that all bodies composed of an atom of base and an atom of oxygen have more or less the characters of sub-oxides. It appears, moreover, certain, that the atoms of the stronger acids and bases contain more than one atom of oxygen. Since the number of simple atoms in a compound atom must necessarily influence the form, and consequently the properties of the latter, we have a right to suppose that oxides which contain the same number of atoms of oxygen, have at least some general properties in common which distinguish them from those that have either a greater or a less number. Thus, as we have reason to presume that the oxides which contain only 1 atom of oxygen have the weakest affinities, we find a whole series of more strongly marked salifiable bases, in which the number of the atoms of oxygen must be twice as great as in the former, and therefore it is probable that all the stronger bases contain two atoms of oxygen. Those which contain three atoms of oxygen, on the contrary, are weaker, and many of them may even be electro-negative with respect to some of the electro-positive oxides;"* that is, act as acids.

Amongst the stronger bases, Berzelius includes all the alkalies and alkaline earths, the protoxides of lead, silver, cadmium, iron, copper, tin, zinc, &c. but alumina, silica, and some others, as may be seen in the table at the end of this abstract, he considers as containing 3 atoms of oxygen. Now, having laid down this arbitrary law, two consequences follow, which the student must keep in mind, or he will be liable to fall into error when he endeavours to translate the formulæ into common language, or compares Berzelius's atomic weights with those of English authors. The first is, that to preserve the proportion between the weight of the oxygen and that of the base, as found by analysis, Berzelius has been obliged to double the weights of the atoms of all those elementary substances whose protoxides he considers as containing two atoms of oxygen, and to treble those which contain three. Thus the protoxide of lead, according to the table (see oxidum plumbicum) contains per cent. 92·83 lead, and 7·171 oxygen, and 7·171 : 92·83 :: 100† : 1294·5; but the weight of the atom of lead in the table is just double that number, viz. 2589. Again the composition per cent. of alumina is aluminum 53·3, oxygen 46·7, and 46·7 : 53·3 :: 100 : 114·11; which is just one-third of the tabular weight of the atom of aluminum, viz. 342·33.‡ Hence the expression $\ddot{C}\,\dddot{S}^2$ means the neutral sulphate of lime, and not the bisulphate which at first view we should probably suppose it to represent, for as the lime contains two hypothetical atoms of oxygen, the salt must also contain two hypothetical atoms of acid, or the canon would be violated which requires that the oxygen of the acid should be a multiple by a whole number of the oxygen of the base. Hence when we find such a symbol as the preceding, and wish to read it correctly, we must remember that all the atoms are doubled, and consequently represented by numbers, which, to reduce them to those of English authors, must be divided by 2. In like manner in the expression $\dddot{Al}\,\dddot{S}^3$ (sulphate of alumina), all the atoms are trebled. As three to one, according to Berzelius's views, represents a neutral salt, so an *equal* number of atoms of acid and base represents a salt *with excess of base*; and accordingly we have $\dddot{Al}\,\dddot{S}$, signifying sulphas trialuminicus.

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* Essai, p. 114, et seq.

† The weight of an atom of oxygen.

‡ To reduce Berzelius's numbers to Thomson's, divide by 100; and to reduce them to Brande's, multiply by $\frac{8}{100}$. Berzelius's number for alumina accords pretty nearly with Thomson's, but not with Brande's or Phillips's. I believe the former to be correct.—C.

We have not yet quite done with these *simple* views; another formula remains to be noticed, namely, that of $\dddot{Al}\ \dddot{Si}$. After what we have just shown respecting the sulphas trialuminicus, the reader will probably suppose that this expression means silicias trialuminicus. No such thing; it is the neutral compound, silicias aluminicus. But with respect to this inconsistency, hear Berzelius's own confession.

"I must here point out a little inconsistency which I have committed in the nomenclature of the silicates, by applying the term *silicias* to combinations in which the oxygen of the base is equal to that of the silica. The analogy of silica with acids containing 3 atoms of oxygen would require that this appellation should be given to those compounds in which the oxygen of the silica is three times that of the base. It is evident that these are the true neutral silicates, and that the first are salts with excess of base, since the alkalies, by decomposing a silicate with the assistance of heat, always reduce it to that point at which the silica and the base contain equal quantities of oxygen. However, as the study of the silicates belongs principally to that branch of chemistry which is chiefly applied to mineralogy, and as the nomenclature of the numerous degrees of saturation of silica becomes much more easy by this method, I have thought it right to adopt it.*

Now really if the symbols are intended "to facilitate the expression of the proportions of the elements of chemical compounds, and to enable us to state briefly and *easily* the number of elementary atoms which any of them may contain," we can hardly conceive any contrivance less calculated to answer its purpose! For here in the very same table we have two expressions of precisely the same kind, denoting two very different sorts of compounds, $\dddot{Al}\ \dddot{Si}$, representing the neutral silicate of alumina, and $\dddot{Al}\ \dddot{S}$ a sulphate of the same base.

Again $\dot{Ca}^3\ \dddot{Si}^2$ denotes one natural salt, silicate of lime, and $\dot{Ca}\ \dddot{S}^2$ another equally neutral compound, sulphate of lime.

The formula $\dot{Ca}^3\ \dddot{Si}^2$ represents, as we have just stated, the scilicate of lime, and $\dot{Ca}^3\ \dddot{Si}^4$ the bisilicate; the acid of the first containing the same number of atoms as the base, and that of the second twice as many, so that these *brief* and *easy* statements require the reader to multiply the dots over each letter by their respective exponents, and then compare the ratios of the products before he can tell whether the salt be neutral, super-acid, or with excess of base. This indeed is easy enough in the two instances just mentioned, but it will probably require some reflection before the reader perceives the meaning of $\ddot{\mathrm{F}}\mathrm{e}^{3}\,\dddot{\mathrm{S}}^{4} + 6\,\dddot{\mathrm{F}}\mathrm{e}\,\dddot{\mathrm{S}}^{2} + 72\,\mathrm{Aq}$, and finds out that it means *sulphas biferroso-ferricus cum aquâ.*

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* Essai sur la Theorie des Proportions Chimiques, p. 170.

[*To be continued.*]

ART. XXIX. *Remarks on Mr Colburn's Theory of Parallel Lines, &c.* By THEODORE STRONG, Professor of Mathematics, &c. in Hamilton College.

I HAVE noticed in the Boston Journal, of October 1825, a New Theory of parallel lines, by Warren Colburn, Esq. which I think inconclusive for the following reasons: *First.* Mr Colburn defines parallel straight lines, to be those which are so situated, as to be equidistant throughout their whole extent. Now it seems to me that Mr Colburn ought to have shown, that this definition is not inconsistent with the definition of a straight line; in other words, that it is possible for two straight lines to be equidistant throughout according to his definition; my views may be illustrated as follows: (see Mr Colburn's Fig. 2d.). Let AB be a straight line, and suppose that CD is another line which is such that the perpendiculars drawn from CD to AB shall always be equal to each other. Now I do not perceive that Mr Colburn has any where shown that CD must, according to this construction, be a straight line. But perhaps I shall be told, that it is so evident as to require no proof. I answer that it appears no more evident to my mind, than "the disputed proposition," which Mr Colburn ultimately endeavours to prove, by the aid of his definition, &c.

Second. Granting, for the sake of argument, that the definition is perfectly unexceptionable. I think that the proof of Mr Colburn's first proposition, in which he undertakes to show that the alternate angles &c. are equal, is inconclusive. For he assumes that the perpendiculars HK, IL, are equal, (see his Fig. 2d.) HK denoting any perpendicular from AB to CD, and IL in like manner, denoting any perpendicular from CD to AB. I do not see how this follows from the definition, for supposing that the perpendiculars from CD to AB are always equal to each other, how does it appear, that the perpendiculars from AB to CD are also equal to each other? But supposing that these perpendiculars are equal to each other also, where is the proof that the perpendiculars to AB are equal to those drawn to CD. I do not find any satisfactory answer to these questions (nor indeed is there any attempted) in Mr Colburn's paper. I conclude, therefore, that he intended his readers should receive them as axioms. But are they more evident, or indeed so evident, as the "disputed proposition?" Once more. It will perhaps be answered to all that has been said, that the definition means that the perpendiculars to AB shall be equal to the perpendiculars to CD. I answer, if this be the meaning, that the perpendiculars to AB must also be equal to each other, and at the same time the perpendiculars to CD must be equal, for the perpendiculars to AB are each equal to HK, and of course must be equal to each other; also the perpendiculars to CO are each equal to IL and of course to each other; therefore since HK = IL by the definition we must have all the perpendiculars to AB equal to each other, and at the same time the perpendiculars to CD must equal each other; I ask now, where Mr Colburn has exhibited any proof of the possibility of this in his investigations. In conclusion I must observe, that it does not appear to me, that Mr Colburn has in the least degree removed the difficulties attending the theory of parallel straight lines.

I have not made the above remarks from a desire to find fault with Mr Colburn's performances. I can assure him that I have great respect for him as an able mathematician, and a man of science; but I thought that the subject of parallels was of so much importance that his communication ought not to pass without animadversion.

Hamilton College, Clinton, N. Y. Dec. 21, 1825.

ART. XXX.—*On the Diameter of Screws.* BY JAMES DEAN, A. M. A. A. S.

EVERY attentive student must have been surprized, on his first examining the investigation of the mechanical powers, at finding that the *diameter* of the screw was not an element in expressing the ratio of the power to the weight necessary to produce motion; and that, other things being equal, the diameter of the cylinder may vary *ad infinitum* without in the least affecting that ratio. That reducing or increasing the diameter to an extreme degree, the distance of the threads remaining the same, will diminish the effect of any given power, will be universally admitted without any very nice reasoning on the subject. But it seems far from being useless to ascertain the *exact* diameter, which, under any given circumstances, will enable a given power to produce the greatest effect.

For this purpose take the equation given by Bossut, *Traite de Mec.* art. 274.

$$Q = P \times \frac{Cp \times (AB + n \times \text{cir. } Cp)}{CQ \times (\text{cir. } Cp - n \times AB)},$$

wherein P represents the weight acting in the direction of the axis, Q = the power necessary to balance it together with the friction, CQ = its distance from the axis, AB = the distance of the threads taken parallel to the axis; Cp = the radius of the cylinder, and n = the ratio of the friction to the pressure, which, between the same surfaces, is found by Amontons, Parent, Coulomb, &c. to be very nearly constant, and commonly about one third. In order to find the value of Cp, when Q is a minimum, the other quantities remaining constant, P and CQ being omitted, as affecting all the terms equally, let $Cp = x$, $AB = h$, and $\pi = 6{\cdot}28$, &c. and the fraction will become $\frac{hx + n\pi x^2}{\pi x - hn}$, which has its least value when its differential,

$$\frac{h\pi x\,dx + 2n\pi^2 x^2\,dx - h^2 n\,dx - 2hn^2 \pi x\,dx - h\pi x\,dx - n\pi^2 x^2\,dx}{\overline{\pi x - hn}^2}$$

$= o$, whence $\pi^2 x^2 - 2hn\pi x = h^2$, and πx (the circumference of the screw) $= h \times (n + \sqrt{1 + n^2})$, whence the diameter may be computed as usual.

From Bossut's formula it appears that when $hn = \pi x$, that is, when the circumference of the screw : the distance of the threads :: the friction : the pressure, the denominator becomes o, and therefore the power Q must become infinite, or the effect o. When this is the case, if h, or the perpendicular height of the inclined plane, formed by one revolution of the thread be taken $= 1$ or radius, then $n =$ the circumference of the cylinder, or base of the plane, or cotangent of the angle contained between any thread and a plane, perpendicular to the axis. Let this angle be called a, then $n = \text{cot.}\, a = \frac{\text{cot.}^2 \frac{1}{2} a - 1}{2 \text{ cot.} \frac{1}{2} a}$, by an. trig. whence $\text{cot.}^2 \frac{1}{2} a - 1 = 2 n . \text{cot.} \frac{1}{2} a$, and $\text{cot.} \frac{1}{2} a = n + \sqrt{1 + n^2}$, which is precisely the value of the circumference given by the differential calculus when the effect is greatest, so that the angle of inclination at which the greatest effect is produced is exactly half of that angle which, with the same degree of friction, is the limit of all effect.

It hence appears that screws are commonly made much too large in proportion to the distance of the threads, to produce any given effect with the least power. Thus, suppose it be desired to produce a pressure equal to ten tons with a screw whose threads are one inch asunder, lever eight feet long, and friction one third the pressure; if the screw be six inches in diameter, the power must exceed 275lb.; if the diameter be only two inches, any power greater than 122lb. will be sufficient. If the friction amount to half the pressure, the dimensions still remaining unaltered, the powers corresponding to the same two diameters will be 398 and 168lb. respectively. The least powers with these two degrees of friction would be 72lb. and 98lb., and require diameters only 0·4416 in. and 0·515 in. respectively, but no substance known, when reduced to even twice these dimensions, would have strength enough to resist the torsion. It appears therefore that a screw, as an engine of force, with any given distance of thread, ought to be made as small as the strength of the material will admit. In order to ascertain by computation the middle course between wasting materials, labor, and power on one hand, and hazarding the failure of the machine on the other, we may adopt the conclusion arrived at in the En. Br. & Ed. En. "that in resisting torsion the whole lateral cohesion is exerted at $\frac{1}{4}$ the radius of the cylinder from the centre." The notation of Bossut's formula being retained, and S being put for the lateral cohesion of a square unit of the substance used, the area of a section is $\frac{\pi x^2}{2}$, and its strength $\frac{S \pi x^2}{2}$, then $CQ : \frac{x}{4} :: \frac{S \pi x^2}{2} : \frac{S \pi x^3}{8 CQ} =$ the force of the cohesion at the end of the lever, which being put $= Q = \frac{P}{CQ} \times \frac{hx + n\pi x^2}{\pi x - hn}$, and the equation reduced gives $x^3 - \frac{hn}{\pi} x^2 - \frac{8Pn}{S\pi} x = \frac{8Ph}{S\pi^2}$, in which if the known quantities, including P and S, are properly substituted, the value of x may be safely adopted for the radius of the cylinder.

But when the exertion of great force or the weakness of the material renders a large cylinder indispensable, the distance of the threads should be greater also, because in that case increasing the distance of the threads does not require by any means a proportional increase of power. Thus with the six inch cylinder abovementioned with the friction of one third, increasing the distance of the threads from one to two inches requires an increase of power less than one sixth, viz. from 275lb. to 319; and with a friction of one half, the increase is only one eighth, or from 398 to 448, which may generally be compensated by using a longer lever while the action will be much more prompt and rapid. And less than double the power will produce the same effect, though the distance of the threads be made equal to the diameter of the screw, or six times their former distance. The reason of this will be obvious, when it is considered that in this case the whole power except 37lb is employed in overcoming the friction, and that at such a small angle of elevation the pressure, and consequently the friction, is but slightly varied by the variation of that angle. And with the same distance of the threads, if the diameter receive successive increments the ratios of the corresponding powers will continually approximate to the corresponding ratios of the several diameters, because the effect of the friction, which absorbs most of the power, is proportional to its distance from the fulcrum.

But after all it is far from being eligible in practice always to give screws the shape above described, especially in the fastening of instruments and machines where the object is not to produce but to prevent motion, because if the distance of the threads is greater than n times the circumference, the friction of the screw will not prevent it from running back. On this account it may not be improper to investigate the figure of a screw so formed that the friction shall always hold it where the power leaves it. Let it be required therefore so to shape a screw that, with any given degree of friction, the power necessary to turn it forward shall have a ratio to the power necessary to turn it back, equal to the ratio of the two given quantities $f : b$, f being of course larger than b. As the length of the lever to which these powers are applied cannot affect their ratio to each other, they may be considered as applied at the surface of the cylinder, and this surface may be further considered as extended on a vertical plane in which case one revolution of the thread is the length of an inclined plane, the circumference of the cylinder is its base, and the distance of the threads its height. Bossut, in art. 273, gives the force which acting parallel to the base (as it always does in the screw) just balances the relative gravity and the friction $= \frac{P \times (HI + n \times GI)}{GI - n \times HI}$, where $P =$ weight, $GI =$ base, $HI =$ height, and $n =$ the ratio of the friction to the pressure. But in order to express the greatest force which, acting in the opposite direction, would not drag the body down the plane, HI in the preceding equation in Bossut, from which the above expression is derived, must be taken — because the force PC or $P \times \frac{HI}{GH}$ or relative gravity, instead of opposing the power, assists it, and the force PM or $Q \times \frac{HI}{GH}$ reversed instead of increasing, now diminishes the pressure which produces the friction or resisting force. With this change the greatest force which (acting parallel to the base and from the summit towards the foot of the plane) will not drag the body down the plane, will be found $= \frac{P \times (-HI + n \times GI)}{GI + n \times HI}$. Let now $HI =$ rad. $= 1$, $n =$ cot. a, as before, and $GI =$ cot. z, z being the angle at the base of the inclined plane, on which, with the given degree of friction, n, the forces forward and backward will have the required ratio $f : b$, and dividing by P, we shall have

$$f : b :: \frac{\text{cot. } a \,.\, \text{cot. } z + 1}{\text{cot. } z - \text{cot. } a} : \frac{\text{cot. } a \,.\, \text{cot. } z - 1}{\text{cot. } z + \text{cot. } a}$$

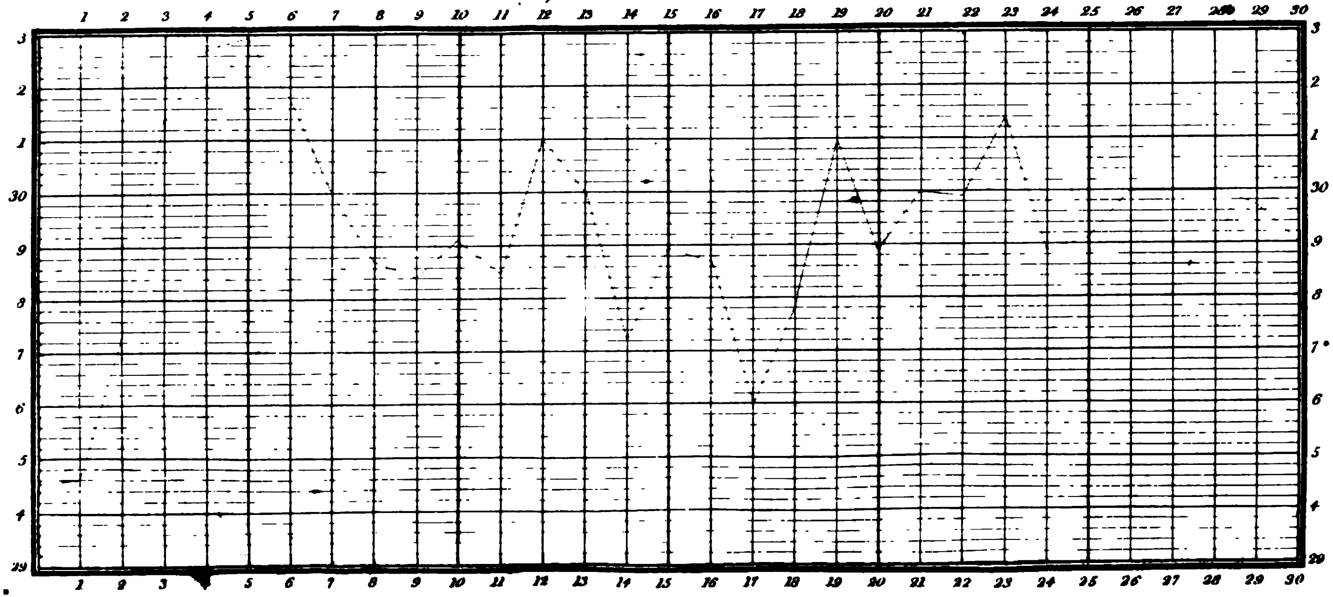
$$:: \text{cot. } (a - z) : \text{cot. } (a + z) :: \text{sin. } 2\,a + \text{sin. } 2\,z : \text{sin. } 2\,a - \text{sin. } 2\,z.$$

Then $f + b : f - b$ :: sin. $2\,a$: sin. $2\,z$, whence the base GI or circumference of the screw, and thence its diameter, may be easily found.

By the diameter of the screw or cylinder is always meant the arithmetical mean between the greatest and least diameters of the bearing surface of the thread. The screw is also supposed to have a square thread, or at least that the section of its bearing surface with a plane passing through the axis is a right line and perpendicular to the axis. But when the thread is in the form

Table showing the range of the Barometer at Washington. Nov. 1825.

Eng^d for M^r Wallenstein's paper



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of a triangular prism, or rather when the section above mentioned makes an oblique angle with the axis, the pressure and of course the friction is increased in the ratio of the sine of that angle to radius, so that $n \div$ its sine must be taken instead of n through the whole investigation. It is also supposed that there is no pressure except on the threads of the screw; in practice however there is almost always a pressure at the other extremity equal to the weight raised or effect produced, and the increase of power required by the friction resulting from this pressure is proportional to the mean distance of its action from the axis of the screw. The introduction of this consideration would produce conclusions differing still more from the common practice than those drawn in the fourth and fifth paragraphs. In most of the instruments called vices this pressure is distributed over a circular space surrounding the body of the screw, and the friction, thus remote from the axis must absorb a considerable portion of the power; but in the presses, it is commonly confined to the extremity of a pivot, and its retarding effect must be comparatively trifling.

ART. XXXI.—*On Barometrical Observations in relation to the Agitation of the Atmosphere and on the Temperature of Washington.* By JULIUS DE WALLENSTEIN. Read on the 28th January, 1826, before a Scientific Institution at Washington.

Mr President and Gentlemen,

AFTER a steady and scrupulous attention to the various phenomena of the atmosphere, the votary of natural science is irresistibly seized with a desire of drawing, from persevering and minute observations, some general result that may contribute to the advancement of science. But Meteorology is yet very far from giving sufficient data for more than a plausible theory of even a single one of the numerous departments of physics to which it extends. After many years of close application in investigating the variations of temperature, all that can be affirmed is, that in the course of several years, the irregularities compensate themselves in such a way, that the sudden and prodigious increase of heat or cold, for a greater or less number of weeks, does not affect the degree of temperature, which nature seems to have allotted to every portion of the earth. But whatever attention the generality of observers may have paid to the changes of the atmosphere, probably very few have fulfilled all the conditions, which, in the progress of their investigations, they discovered to be indispensable for obtaining satisfactory results. One may have forgotten to examine the construction of his thermometer, to compare it with a standard of unequivocal precision, or to go through the minute process pointed out by experience and theory, for ascertaining the regularity of its indications. Another may have neglected to bring the instrument into the current of the prevailing wind. The hygrometer requires a frequent repetition of the elementary experiments, by which its scale was determined. The instrument, invented by Saussure, is generally very troublesome, and becomes useless in strong agitations of the air. The greatest caution is necessary when the hair is to be cleansed, and this operation must be frequently repeated. A barometer, that delicate and vexatious companion in a journey, is, upon the whole, the least troublesome instrument during a tranquil residence.

When thus, after a long series of meteorological observations, the various results crowd themselves, in separate columns, under the eye of the examiner, he is, at once, discouraged by the uncertainty of his operations and the disproportion between their utility and the labour they have cost.

Division of labour, which has become so productive of beneficial results in many branches of industry, and is nothing but method and classification, presents its advantages also in science. If in the actual state of Meteorology, it is almost impossible to combine, into one great result, the various modifications observed in the atmosphere, with the instruments adapted to the examination of its constituent parts, and their changes; let us, by dividing the task, endeavour to remove some of the difficulties that present themselves.

The theory of winds is one of the least perfect parts of natural philosophy. Even their course or direction and their velocity have yet been but little attended to. The causes that influence them are connected with facts, the investigation of which constitutes the highest branches of physico-methematical sciences. Without taking into consideration the action of gravitation, the rotation of the earth, the specific weight of the fluids, and the changes which caloric produces on them, the phenomena of the Trade wind could never be explained; the dessicative power of certain winds would be a source of error or a mystery, if the nature of caloric was not understood; and without an intimacy with the constituent parts, the properties and the effects of the fluid that surrounds us, and, at least a general knowledge of the different formations of our globe, we could not conceive of the origin of vapours, rain, fogs, snow, &c. that succeed each other, sometimes so rapidly, around us, nor explain subterraneous springs and rivers, and by them again, in part, some of the atmospherical phenomena. The sun and the moon act doubtless on the atmosphere. Their combined action is visible and can be rigorously calculated, as far as it extends on the ocean, and the adjacent land, in bays and narrow channels. It is true that the opinion of Galen with respect to the influence of the lunar phases on the crisis of certain distempers, is now disregarded. Able physicians of our day fear only the effect which such opinions could have on patients whose delicate nervous system disposes them to receive every impression that strongly excites their feelings. Olbers, the great astronomer and physician, never thought himself justified to ascribe any influence to the moon on epileptic and nervous disorders and other serious distempers. Yet he does not deny that the relative situation of the sun and moon may have some influence on a few rare maladies.

Returning to the subject, whose obscurities I have presumed to excuse, by mentioning those which cover many other branches of science, and by referring to various examples of their branching out and their real concatenation, I wish to state that in my humble opinion, an assiduous observation of the phenomena connected with the variation of winds, seems to me not one of the least interesting branches of natural philosophy. If the causes by which these perturbations of the air are produced, are yet too little known to throw light on the influence they exercise on all that bears the principle of life and seminal faculties, on our activity, our comfort, and our health, the effects require so much the more attention.

Besides in this department of meteorology, a hope may be indulged that the observations may lead to a series of facts somewhat regularly connected with another ramification of the science. If a great number of observations should show a simultaneous increase or diminution of the height of the barometer, if, during the same current of air, most commonly the mercury of that instrument has varied but little or not at all, if a considerable variation in its range had been followed by a corresponding decrease; these phenomena would deserve the attention of a philosopher, besides their possible reference to good or bad weather, as a means of elucidating hereafter, more and more, the relative dependance of the operations of nature, and of counteracting the errors and illusions, to which we are liable by the imperfections of our organs, our education, and our passions. The prevailing winds have, to be sure, been noted by almost every body that has kept a meteorological journal. In separating from any one of those tables the barometrical observations, in connexion with the corresponding remarks on the state of the heavens, one could bring under a single point of view the possible relation of the pressure of the atmosphere, with its agitations. But in experimental philosophy, facts are only well ascertained, when the observations or experiments are made with an express and exclusive purpose. Surely the temperature is a *function* in every calculation of barometrical changes, but the pressure of the atmosphere is also a *function* in that of the effects of caloric. But like the ramifications of any other department of natural science, they must be separately examined and separately described, although they are afterwards reconsidered in combination with other phenomena.

The method I have adopted, in the annexed table, is withal, in one point of view, but a mean of observing how often certain winds have prevailed in a given time. Should observations be continued with that stated purpose, the view of the subject taken up by the great mathematician Lambert, in the Tranactions of the Academy of Science of Berlin, for 1777, may be brought to a greater certainty, and a formula for determining the force and frequency of given winds, may not be considered much longer as merely an ingenious speculation.

M. de Buch, the scientific friend of Alexander de Humboldt, and the author of a very instructive tour into Lapland, and of many valuable disquisitions, has examined the subject under consideration, in a Memoir addressed to the Berlin Academy in 1818. Of the southwest winds, he observes, that taking the case of their prevalence during three days, one was accompanied with rain. Of 17 days of the northeast wind, one only was misty. The winds from northwest to south, were generally more accompanied with fogs and mist than any other winds. During two days and an half of southwest wind, one was rainy or snowy, and, on the contrary, of 8 days easterly wind, seven were remarkable for dryness. He gives the following table to show the comparative height of the barometer, under the influence of the 8 winds, from north round south, to northeast.

N.	NW.	W.	SW.	S.	SE.	E.	NE.
Lines. 336·32	335·85	335·13	333·61	333·06	334·55	336·36	336·62

In comparing this table with the following, it will be seen, that under the same winds, the barometer is less elevated when it snows.

Lines.							
33325	334·37	333·62	331·93	330·76	332·21	333·38	333·75

The difference of these two tables is as follows:

3·07	1·48	1·51	1·68	2·30	2·34	2·98	2·87

By a prevalence of the north, northeast, and east winds, the difference amounts sometimes to three lines, and the barometer is then as high as it is, on an average, when the wind is from the south. However, snow is more common with north winds, but they operate only, when the snow has already begun to fall; the snow itself owing most probably its existence to an encounter of the fresh air of the north with a misty one from the south. This is exemplified by the following observations quoted in M. de Buch's Memoir.

			Barometer. *Lines.*	Wind.	
1783	March 2,	at 10 o'clock	328·11	SSW.	
"	" 3,	7 "	330·50	NNE.	snow.
"	" "	2 "	332·7	N.	
"	" "	10 "	334·8	N.	

When the northern wind becomes steady, the snow falls in small flakes. During the winter, the greatest elevation and depression of the barometer are separated only by a short interval of days, and M. de Buch has observed that the mercury rises more rapidly than it falls. On the 12th March 1786, it snowed with a south-southwest wind, the barometer was at 333 lines, and the thermometer between — 1 and + 1°. M. de Buch explains these phenomena by saying, that the SSW. which kept the thermometer at so low a degree, prevailed in the upper region of the atmosphere, while nearer to the earth, a northern wind exercised its influence. In the upper regions the vapour was transformed into rain, which in its fall turned into snow under the influence of the wind which then prevailed. Effects of contrary winds, in the upper and lower regions of the atmosphere, are the most strikingly observed in mountainous countries. The snow melts often at an elevation of 3000 feet, on the declivity of a mountain, while in the valley the ground is frozen.

The author, whose observations are here quoted, has remarked, that in the places where he has made his investigations, the mean temperature is never lower than — 4·9° of Reaumur (21° Fahr.) when snow has long continued; and the only variation he mentions is a fall of snow on the 14th of January 1808, somewhere in Norway, under a temperature of — 10·3° (8·8° Fahr.) during an easterly wind. The snow was dry and in small flakes.

I beg to observe, that these citations are, as I find them among my notes; and I regret not having it in my power to lay before you M. de Buch's valuable Memoir, rather than so scanty a specimen as I have presented of what seems to me a fine example of philosophical analysis.

Before I present to you, Gentlemen, my own observations on the climate, where we live together, you will, I hope, permit me to call your attention on the investigations made by a traveller, whose labours are known to the world, who first has described that cluster of islands, the name of which is dear to your heart; who has conducted an expedition round the world with the energy of Cook, the accuracy of Vancouver, and a philosophical mind, not unlike that of Banks and Foster; and whom I, an humble individual, am proud to call my friend. Amongst the researches of this scientific circumnavigator and hydrographer, those on the direction of winds may not yet be known to you, Gentlemen, for I know only of two copies of the valuable work, that contains them, having reached this country a short time since, and where Commodore Krusenstern has, for the first time, supplied the deficiency left in that branch of natural philosophy by Daprés, Dalrymple, Romme, and Horsburgh.

Commodore Krusenstern presents, in the introduction to his Hydrographical Memoirs, accompanying his Atlas of the Pacific ocean, the following outline of a treatise on the winds and currents—"In the Pacific ocean from latitude 30° to the pole the "variable winds" are generally from the northwest and southwest. In the straits of Behring, the north and south winds are the most general. On the coast of Kamschatka, the prevailing winds, from June until the middle of September, are from southwest and west; and the journals of La Pérouse and other navigators prove, that Romme erroneously indicated those of of east and southeast as the most frequent, in the interval above mentioned. On the Kourili islands, according to Capt. Golownin, (whose firmness is likely to be more known to you than his inquisitive mind,) the weather is foggy, when the wind blows from south-southeast to south-southwest, but so dry that the sun is visible. The east and northeast winds are accompanied with rain and storms. The north-northeast and north-northwest bring a clear sky and cold weather. Commodore Krusenstern found on the 31st March, 1805, his thermometer (Reaumur) at 1½°

below zero (28·7° Fahr.) during a wind of north-northeast, which was accompanied with snow. In winds from north-northwest to west-southwest, the weather is cold and the air dry and foggy; and during those from south-southwest to west-southwest, the skies are clear and but seldom clouded, and the temperature remarkably high.

"In September and October, Commodore Krusenstern found on the Oriental coast of Japan variable winds, but more frequently those from the north than the south. On the east and northeast coast of China and on those of Corea, and in the gulf of Tartary, the north and northeast winds blow during nine or ten months, and they turn seldom to the south. In the sea of Sachalin, the winds vary from south to north, according to observations made during 8 years at Ochotzk. With respect to the sea of Ochotzk or the northern portion of the Sachalin sea, the prevailing winds in June, July, and August, are south and east, not strong, and commonly accompanied with fogs and rain. In September, at the setting sun, the wind goes from the south to the west and northwest, blowing pretty strongly from the latter direction during the night. Towards 10 o'clock in the morning, it blows milder and turns to the south. During the whole month of September, the skies are cloudless. From the middle of October till the beginning of December, storms and strong winds are almost uninterrupted from the east or southeast, and the fine weather begins again, when it blows from the north or northwest. From December until March the winds are constantly north and northeast and the weather is fine. In April and May they blow moderately from the north during the night, and from the south during the day. Consequently, the easterly winds are almost permanent in the sea of Ochotzk.

"On the northwest coast of America, from the straits of Behring to 30° of northern latitude, the winds are variable, though M. de Humboldt presumes that from 65° to 50° they blow half a year from the south-southwest and southeast, and the other half from the north and northeast. The southeast wind is there in every season accompanied with rainy and foggy weather, and the northwest with cold and dryness.

"Capt. Cook found in March, in the 44th degree of latitude, a fresh and constant northwest, which continued until the beginning of the summer, with the exception of a southeast, which lasted, however, only 6 hours, and La Perouse, Portlock, and Dixon, did not experience the south winds in the summer. According to Vancouver and the Spanish navigators, the north and northwest are the most prevailing. Hence the difficulty which is experienced on that coast for advancing to the north, and which can only be remedied by profiting of the east and southeast land breezes, which are of longer duration and stronger, than the sea breezes. Vancouver states that north of Cape Mendocino, the weather is generally bad, and that south of that promontory, the winds are moderate and the skies clear, and that during the winter, the south winds are moderate.

"In Nova Archangelok (57° 2′ latitude N.) the prevailing winds, during the winter, are from the east, accompanied with rain and snow. At the beginning of December gales and storms are frequent.

"On the coast of California, the northwest wind prevails during the summer until October. In November it changes into southwest and southeast with rain, and the weather clears off by a northwest. In October a hot northeast, that prevails for a short time, produces cough and pleurisies. In December and January the mountains begin to be covered with snow, but only for a few hours. In March and April, the winds become variable. During summer and winter thick fogs are frequent. Commodore Krusenstern proceeds with the same minute attention, describing what he calls the 2d and 3d zone, or the portions of the great ocean which lie between the 30th degree N. latitude and 30th degree S. latitude. The trade winds, whioh prevail in that space, are influenced by the direction of the adjacent coasts, as for instance on those of Peru and Chili, where they are north and south, in consequence of the coasts running in that direction."

But, Gentlemen, it is not to my purpose to follow my scientific friend further in that part of his investigations, howsoever interesting they may be in relation to a different object. I will only add, that the facilities which the constancy of the southerly winds seem to afford for the navigation towards the north, on the last mentioned coasts, are counteracted by currents which drive to the south, and that farther to the north a similar current carries to the west. Between the 30th degree latitude S. and Cape Horn, the regular winds from north and south begin again. Near Coquimbo (30° lat.), the north and northwest winds blow during two or three months in the course of a year, and near Valparaiso (33° lat.) the same winds prevail from May to September, accompanied with rain and fog, but not violent; and during the rest of the year the weather is dry, the winds blowing constantly and strongly from the south. Near Cape Horn the winds that blow most constantly are from the southwest and northwest. Near Cape Horn and in the Sachalin sea, Commodore Krusenstern observed a fall of the mercury of nearly 9 lines, though the state of the atmosphere was not altered, and the barometer remained stationary until his departure from those waters, and then the barometer rose again.

According to the manner in which I consider the subject, I humbly believe, that in order to remove the uncertainties which surround it, the principal cause of winds must before all be clearly ascertained, and as soon as we are satisfied of its real nature, the irregularities of its influences must be gathered in as great a number as possible. Hence in a series of remarks on the currents of air prevailing the most in this country, a glance may, and perhaps must, be thrown on the corresponding phenomena under different latitudes and distant meridians.

Winds are the effects of an alteration in the specific weight and elasticity of the air; an alteration, however, which does not pervade equally all its parts. Amongst the latter, we must particularly take in account the atoms, the extremely small particles of water which fill the atmosphere and become only distinctly visible to our eye, when their increased density puts them under the influence of the general action of gravitation. According to these premises, the currents of air, are, by their nature, incessantly changing. Durable winds are, then, but exceptions. They are produced by the difference of the increase of heat in two terrestrial points. The air the most heated, is the most dilated, and its specific weight is diminished; but the circumambient air of less temperature, would take the place of that which has comparatively a greater specific weight, until this air becomes, like the former, dilated by the action of heat. On the contrary, when a portion of the atmosphere becomes colder, its dilatation is diminished, and a warmer air takes the vacant place. This explains the reflux of southern air to the north, at the beginning of autumn or the cold season.

Winds coming from the summits of high mountains are always cold. Even in Guinea, the northwest wind, which blows from the mountains of the interior, is cold and dry. Nevertheless the summits act, by affinity on the product of the evaporation of the sea, and arrest clouds. But the abundance of humidity, which hence results, operates only as a generative cause of water spouts and rivers, according to the geological character of the mountains. Conflicting winds first occasion calms, and afterward storms and tempests. The clouds of different species of electricity, which they possess, produce thunder, the cessation of which is generally accompanied with a change of wind. There are further exceptions, as, for instance, the winds which are peculiar to certain terrestrial regions, as the Samiel wind of Egypt, Persia, Arabia, and Syria, the north wind of the Gulf of Mexico, the hurricanes of the Cape of Good Hope, and the "Typhon" of the seas of Japan and China, all which have peculiar symptoms, unequal though prodigious strength, and are even distinguished, it seems, by colours. Periodical and trade winds are in part explained by what has been said on the general cause of the currents of air. A more complete *explanation* of these and of tropical winds, as far as plausible speculation may be so called, would require more time than I can command.

In the middle latitudes between the poles and the equator, the winds change oftener than under the tropics. In fine, several winds blow very frequently at the same time in successive regions of the atmosphere. While a strong current agitates the air of a mountain, the inhabitant of the valley enjoys a calm air, and "vice versa." Winds, chiefly those which are dry, accelerate evaporation. This can be explained by their uninterrupted contact with the waters, or the interstices which, under the influence of the elasticity of caloric, they present to vapours, or by the agitation they produce in the liquid atoms. However, an agitated atmosphere is more desiccative than a tranquil air. The vapours, as soon as formed, are carried away, and the air itself becomes not saturated with humidity and does not lose its dissolving force. Saussure remarks that the ascension of fogs and clouds which are composed of vapours is coeval with the rise of the barometer, in consequence of the increased density of the air near the earth. The moist vapours seek then in a region more elevated, an air which is more in equilibrium with them. According to that great philosopher, the smoke and the vapours which surround the summits of Vesuvius and Ætna, rise and fall contemporarily with the mercury of the instrument that indicates the pressure of the atmosphere, and these movements are even considered as prophetic indications. The influence of winds on the temperature extends itself to a greater height than the influence of the direct rays of the sun. The causes which influence the barometer begin to act on the uppermost regions of the atmosphere. In a great part of Europe, the west winds are generally humid and the easterly, on the contrary, very dry. In Switzerland the south and southwest winds are warm and bring commonly rain, and the dryest wind is that from the northeast. The coldest wind in Europe comes from the north. Muschenbroek (according to a quotation of Saussure) has found that in Belgium, the easterly winds are dry, the sky being clear, but when they follow westerly winds, they are accompanied for a while with rain. In northern Europe winds which tend to turn from north to northeast, pass before through all the other points of the compass; and those which tend to turn from north to west, accomplish it without any deviation.

Let us now collect some facts on the relation of barometrical variations with winds. The source from which I derive them, is again the work, already often quoted, of the great naturalist of the Alps. Generally an increase of heat produces a fall in the barometer. The mercury of that instrument stands higher during the night than the day, in consequence of the diminution of heat. The variations of the barometer are greater in winter* than during the summer in the northern climates, in consequence of the considerable difference of the temperature of the divers regions of the atmosphere, and the proximity of cold and warm air. Local causes produce sometimes rapid and irregular movements of the barometer. When the atmosphere is cooled a few degrees by rain, the barometer rises suddenly, says Saussure, half a line and even a line, without the co-operation of any general cause, and this because a single column of air being condensed by the cold, the other columns which have not undergone that process, extend themselves over it and increase its bulk. The greatest variation of the barometer happens within the interval of a few hours in places distant more than 300 miles. Saussure ascribes this phenomenon to the prodigious velocity of the winds in the upper regions of the atmosphere, where, as it has been already observed, the causes of the barometrical changes are incipient. I will conclude these summary remarks with an observation of Saussure on the exactness of barometrical predictions with respect to rain. In that view, he recapitulates and compares the results presented by several other great naturalists. "The Marquis Voleni has found," says he, "that of 1175 times that, in the course of 12 years, it had rained in Vadua, only 758 produced a fall of the barometer; consequetly of 1000 observations only 645 proved true, and the influence of rain on the barometer seems not great. Van Swinder had collected as many false as true predictions of that nature, but in calculating the observations of one year, according to Mr Horsley's method, he has demonstrated the influence of rain on the barometer, and discovered that in 1778 and 1779 the mean height of that instrument, during *rainy weather*, was less than the *mean height of the whole year*.

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* From April 1823 to April 1824, I have found that the height of the barometer was, during the summer months, 29·910 inches, and during the rest of the year 29·926 inches. Difference 0·312 lines.

I now beg leave, Gentlemen, to inform you of some of the results of my own investigations with respect to the apparent connexion between the barometrical variations and the reigning winds.

The extensive table I submit to your examination contains observations made during the months of September, October, and November last, and calculated according to the method and the formula, mentioned in the Memoir, which the Philosophical Society has done me the honour to publish in the last volume of its Transactions.* The following recapitulation brings to a still clearer and more comprehensive view, the various results which can be deduced from those observations.†

Average Heights of the Barometer during the Prevalence of given Winds.

Sept. 86 observations.			Oct. 90 observations.			Nov. 89 observations.		
Winds.	No. of obs.	Barom.	Winds.	No. of obs.	Barom.	Winds.	No. of obs.	Barom.
		Lines.			*Lines.*			*Lines.*
N.	8	357·036	N.	9	356·796	N.	12	358·776
NE.	9	356·520	NE.	4	356·444	NE.	2	359·400
E.	4	356·172	E.	5	358·872	E.	3	359·868
SE.	18	355·544	SE.	10	357·264	SE.	8	359·904
S.	17	356·424	S.	29	357·984	S.	26	359·352
SW.	3	354·528	SW.	6	358·880	SW.	4	360·432
W.	5	356·796	W.	4	358·116	W.	8	358·950
NW.	22	356·376	NW.	23	355·332	NW.	26	359·256

I fear I have already too much intruded on your patience by a mention of numerical data, which I know must be sparingly introduced in a sketch like this, which is to be heard before its author can expect to find readers. I submit therefore to you the table which I have mentioned, not altogether without a hope that some of you, Gentlemen, may honour it with your attention, and in this supposition, I would be more gratified to receive information from the conclusions which you may derive from them, than to furnish inferences by my commentaries on my own investigations.

But I owe you my feeble assistance in a task which I know to be a heavy tax on time and patience. I will therefore recapitulate, as briefly as possible, the assertions of an author, whom you will probably consult before you decide on my researches and survey critically the results of my observations.

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* See this Journal, vol. iii, p. 193.

† It is important to inform the reader that these baromtrical observations are reduced to zero of the centigrade thermometer, and corrected for the capillary attraction.

Volney, notwithstanding his unkind feelings towards the country, where he had once found a refuge, and the incompetency in which his want of instruments placed him for acquiring the authority of an observer, often deserves a respectful consideration, when he generalizes data other than his own.

Count Volney asserts, that the north wind is rare in this country, except, says he, in the neighbourhood of Williamsburg. The table annexed to the present remarks warrants in some degree that assertion, but we all know that the author of the "Soil and Climate of the United States" is not correct in saying, that the *north wind is rather misty than dry, and oftener accompanied with clouds, than with a clear sky,* (vol. i, p. 176). The remarks of the author, on the southeast winds, are in accordance with our experience; but the assertion that the south winds are cooler than those from the southeast, is objectionable. However, these differences do not warrant discrediting the author's remarks on the climate of this country; and I am so much the more willing to do him that justice, as I was reluctant to admit his inferences with unlimited confidence.*

In regard to my own observations at a first inspection, or rather omitting to take into consideration the number of *observations* corresponding to every one of the 8 winds under which they are ranged in the annexed table, it would seem that in the course of either of the three months, the height of the barometer was but little affected by the change of the wind. For instance, in September the barometer was at 356 lines (omitting fractions) during 5 of the 8 winds from north to northwest, and again in November it was at 359 lines, at the same number of observations, though not in the same winds. But I would remark, that allowance must be made on account of the number of observations. Thus in September, 9 observations giving 356·52 lines, during a northwest wind, are equal in result to 3 during a south-

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* Observations on wind and weather mentioned in Dr Rush's valuable work, and made in Philadelphia during September and October 1793, show that of 6 days in September 1793, when the wind was north, but one day was cloudy. Three days of northeast wind were likewise cloudy. With southeast winds one day was fair, one cloudy, and one rainy. Two days of south winds were cloudy. Three days of southwest, one day west, and seven of northwest were fair. In October 1793, five days north, two northeast, one of east, one of southwest, four of west, six of northwest winds were fair; two days of north, one of south, and three of southwest winds were cloudy. One wind of the north and two from the southwest were accompanied with rain.

Mr Josiah Meigs states for 1820 (in Washington.)

Winds.	East	1.	Winds.	Northeast	91.
	Southeast	11.		West	108.
	North	13.		Northwest	155.
	South	43.		Southwest	177.

westerly wind. Now a great height of the barometer, resulting from a great number of observations, proves, it seems, that the effect observed in most of these cases is greater, than when demonstrated by only a few observations. The example I have quoted will illustrate my idea: 9 observations, in one wind, indicating 356·52 lines, are equal in result to 3 observations during another wind. Were then, *individually*, the former generally of smaller rate than the latter? No; but smaller quantities were compensated by greater, and these must have been more numerous than the former. Three observations may be accidentally productive of a great numerical result, but 9 observations lessen comparatively the influence of exceptions, and seem to indicate rather the rule.

I have tried various methods to bring this to a clearer view; finally, I have thought that separating the winds, according to the physiognomy and the geographical position of this country, with respect to sea and land, the investigation would at least repose upon an adequate ground. I have therefore compared the barometrical results connected with the winds from W. NW. N. and NE. with those which are conjointed with the winds S. SE. S. and SW. The latter may be called Atlantic winds, and are in the direction of the coast from the Bay of Fundy to the Strait of Pamlico, and the former are, on the contrary, continental winds. Taking then *proportionally*, their corresponding barometrical results, it appears that,

1°. *In September*, the barometer was more elevated during the prevalence of the E. SE. S. and SW. winds, than during the W. NW. N. and NE. winds.

2°. *In October*, it was, on the contrary, considerably more elevated during the W. NW. N. and NE. winds, than during the winds S. SE. S. and SW.

3°. *In November*, it was again, as during September, more elevated during E. SE. S. and SW. than in those of W. NW. N. and NE.

Taking from the observations of November the N. and NE. winds and comparing their barometrical results with those belonging to S. and SW. winds, it will be seen that the latter were considerably smaller; and taking from the same month the easterly and southeasterly winds, for a comparison with the W. and NW. winds, the former will be found accompanied with much greater barometrical heights than the latter.

*During the month of September the barometer stood generally the highest with winds from the north, northeast, and northwest. The average of 39 observations made during the prevalence of these winds, give us a mean height of 356·6 L. It will be seen by the annexed table, that it descended only once to 0·7481 m. (29·453 in.). Fortyseven barometrical observations during the prevalence of the other 5 winds furnish a mean height of 355·89 L.

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* See note, page 288.

The south and southeast winds, in which the barometer was observed 35 times, indicate but a mean height of 355·98 L. yet some of the observations, taken separately, seem to indicate a great rise of the mercury. In the column of the southern winds, the indications are twice 0·760 m. (29·922 in.) and the barometer never fell, during their prevalence, below 0·7481 m. (29·1453 in.). Calculating separately the observations, made under winds from the north, northeast, west, and northwest, we find for 44 observations, an average of 356·65 L. a result which is equal to that of 39 observations, made in the winds from north, northeast, and northwest. 42 observations in the winds from east, southeast, south, and southwest, give but 355·65 L.

Again, during the month of October, the barometer was observed 40 times in north, northeast, west, and northwest winds, and the average result of these 40 observations is 356·67 L. The average of the other 50 observations of that month, in winds from the east to southwest, is 358·2 L.

Finally, during November, 48 barometrical observations in winds from north, northeast, west, and northwest present a mean result of 359·1 L. while 41 observations in the other four winds give 359·9 L. Examining separately the observations of November, when the northwest wind prevailed, one is struck with the uncommon height of the barometer, which rose once to 0·7672 m. (30·394 in.).

Resuming these relative results of 3 months, we discover that in *October*, the barometer was higher in north, northeast, west, and northwest winds, than when the other four winds prevailed: that the contrary was observed in November and September. However, this conclusion must be qualified, on account of the number of observations corresponding to every one of these subdivisions; but the differences are so slight, that they can be neglected in calculations like these. Yet it may be seen by such an analytical examination, how quantities may be combined, in order to furnish useful results, and how useless or productive of errors they are, as long as they remain in no other regularity, than that which their chronological order gives them. According to the happy remark of Haüy, "the Arithmetic of the barometer is a surer guide, than its mere rise and fall."

The following table shows the differences from one day to another, during November, of the mean heights of the barometer.

November, 1825.

Days.	Mean height of the Barom.	Differences.
1	29·461	
2	29·727	+ 0·266
3	30·141	+ 0·414
4	30·277	+ 0·136
5	30·319	+ 0·042
6	30·188	— 0·131
7	30·053	— 0·135
8	29·874	— 0·179
9	29·834	— 0·040
10	29·915	+ 0·081
11	29·850	— 0·065
12	30·103	+ 0·253
13	30·015	— 0·088
14	29·711	— 0·304
15	29·883	+ 0·172
16	29·875	— 0·008
17	29·602	— 0·273
18	29·788	+ 0·186
19	30·083	+ 0·295
20	29·895	— 0·188
21	29·997	+ 0·102
22	29·990	— 0·007
23	30·137	+ 0·147
24	29·878	— 0·259
25	29·916	+ 0·038
26	30·003	+ 0·087
27	30·007	+ 0·004
28	30·010	+ 0·003
29	29·970	— 0·040
30	29·901	— 0·069

The rise and fall of the mercury in the barometer are generally gradual and slow. Quick changes have been observed in north and south winds, viz. from 4 h. P. M. of the 10th of September to 7 h. A. M. of the 11th the rise was 0·0073 m. or 3·404 lin. the wind being northwest and the weather fair. From the 16th of October 4 h. P. M. to the 17th, 7 h. A. M. the change was 0·0053 m. or 2·544 lin. the wind being northwest and the weather fair. From the 1st to the 5th of November, the change was 0·0250 m. or 1 inch, the wind having blown generally from the south with a clear sky.

"The elasticity of the air of the temperate zones," observes M. de Humboldt [Tableau Physique des Régions Equinoxiales, &c. p. 90,] "varies in the same place sometimes as much as 20 lin. French measure, 25·488L.; under the tropics, where the regular winds bring constantly an air of equal temperature, from the 10th degree north to the 10th south of the equator, the elasticity does not vary, on the sea shore, more than 1·4 lin. (1·488 L.) and at 9235 feet above the level of the sea, it does not alter 0·7 lin. (0·744 L.)."

Our observations show that the variations are here more considerable than under the tropics, but less than M. de Humboldt, in his general statement, ascribes to the temperate zone. During November last, the range was 0·961 in. (11·532 L.).

The success of my endeavours to compare the mean height of the barometer at Paris with that in Washington, has been affected by the want of any detailed account of the former, except the "Annales de Chemie et Physique," for 1823, from January until May inclusive. However, taking the *mean* of the *average height* of the barometer during that interval of time at Paris, which amounts to 0·75303 m. (29·646 in.), and comparing it with the mean height of the barometer in Washington during November last, according to my observations, (0·76052 m. = 29·942 in.), it will be found that the mercury stood *here* higher than in Paris by 3·552 L.

From April 1823 until April 1824, I found the mean height 0·760 m. (20·922 in.).

M. de Humboldt [Tableau Physique des Régions Equinoxiales] has supposed in his barometrical measurements the height of that instrument on the level of the sea 0·76202 m. (30·001 in.) at + 25° of the Cent. thermometer. Reducing it to zero of the same thermometrical scale, the height would be

	m.		in.
	0·7602	=	29·931
Average of November last in Washington			29·944
Average from April 1823 to April 1824 in Washington			29·922

M. Biot says [Traité de Physique, Experimentale et Mathématique], that the mean height of the barometer is on the level of the sea 0·7629 m. (30·036 in.), the temperature being 0·8° Cent. (1·4° Fahr.). At Paris, on the level of the Seine it is

0.760 m. (29·922 in.); and according to the observations of Rohault, continued during fifteen years, it varies from 0·7669 m. (30·193 in.) to 0·7496 m. (29·513 in.) by a mean temperature of + 12° Cent. (53·6° Fahr.) [T. 1.p. 95.]

The result of my observations from 1823 to 1824 is exactly in accordance with that indicated by M. Biot for Paris, and by M. de Humboldt for the average height on the level of the ocean. M. de Humboldt says, that this latter is "the result of his observations under the tropics, as well on the coasts of the Atlantic as on those of the Pacific. Bouguer adopted only 0·76022 m. (29·922 in.) and the Spanish Geometer, Don George Juan, 29·800 in. La Condamine thought that if the mean height of the barometer, under the tropics, was not less than 29·841 in. (28 in. of Paris), it differed but little from that quantity."—"My own observations," continued M. de Humboldt, "made with barometers well purged of air by fire, and which had been compared with those of the observatory at Paris, seem to prove that the mean pressure of the air, on the level of the tropical seas, is less than in the temperate zones."

"M. Shuckburg has found the latter to be 0·76300 m. (30·040 in.) and M. Fleurian Bellevue 0·76434 m. (30·091 in.) by 12° Cent. (53·6° Fahr.). This difference of nearly two millimeters cannot be ascribed only to the difference of the mean temperature of Europe and the equatorial regions, so much the less, as in the lower districts of Peru, during the four or five months that the sun is veiled by a thick mist, the thermometer remains at 15° or 16° Cent. (59° or 60·8° Fahr.). This is a problem of as difficult solution, as the horary oscillations of the barometer under the equator."

The importance of barometrical observations needs not be urged upon you. The constitution of the atmosphere, its form, and its state in its successive regions, will only be understood, when a great number of observations, made with excellent instruments, will furnish to great mathematicians the necessary elements for this difficult discovery. Much has been done in that respect, since Pascal applied his powerful genius to this subject, and taught philosophers to determine with a little quicksilver, inclosed in a tube of glass, the various density and pressure of the atmosphere, and in consequence, the irregularities of the surface of the earth; and to navigators to escape dangers which they would not have expected when guided only by the appearances of the skies.

The barometer has caused the discovery of striking distinctive features in the countries situated near to the equator. It was known the height of the barometer diminishes from the pole to the equatorial line.* La Condamine had already mentioned the regularity of the oscillations of the mercury in that instrument, and Halley and Cassini had investigated with sagacity the causes of this phenomenon. But M. de Humboldt gave certainty to this fact. He found regular variations at every hour of the day, not only on the coast of the Pacific, and the valleys of the Amazon, but on places elevated 12300 feet, of Paris, above the level of the sea. These variations are not influenced by the weather, nor by changes of temperature, nor the revolution of seasons, but are exclusively connected with the position of the sun. In certain places under the tropics, a time-keeper could almost be regulated by the state of the barometer. At noon the instrument stands at 338·02 lin., at 12 o'clock at night it falls to 337·88, at the 20th hour it is at 338·16.

In Europe, the climate is too variable to give such conclusive and regular results. Nevertheless the great natural philosopher Ramond has ascertained from observations continued during several years, a variation of the barometer influenced by seasons. There are two *maxima* and two *minima* resulting from these observations, the former near the summer and winter solstices, and the latter in April and September, and the mean height of each month is as the *maximum* shortly after the winter solstice, and the *minimum* in April. M. de la Place states in his "Philosophical Essay on Probabilities," that analysis proves, that these results are probable, and that they are moreover confirmed by other observations. That eminent Mathematician adds: "Time will teach if the months of the barometrical *maxima* and *minima* are coeval in the various climates. These annual and diurnal variations of the barometer are like the trade winds and monsoons, a result of the solar heat, combined with the rotary motion of the earth."

In the Memoir, which I have perhaps already too often quoted, I have mentioned a certain regularity which I have observed in the diurnal oscillations of the barometer, in the interval from the 17th of April 1823, until the 18th of April 1824, and which consisted in the diminution of the height of the mercury from 9 o'clock until 4 o'clock P. M. when the column rises again, till it reaches its *maximum*, for the space of a day, at 9 o'clock A. M. Such observations should be continued for a series of years. In experimental philosophy all causes which act with regularity may be ascertained, and their influence distinctly described by multiplied observations of these events. "And the analysis of probabilities," says M. de la Place, "fixes the probability of the existence and the intensity of the cause."

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* The average of it is a line less on the equator than between the 40th and 50th degree of latitude.

Such is the variety and the importance of the results, to which long observations may lead, that even a natural history of the atmosphere may at some future time be written; I mean a history of its state at different times. Great changes of heat are testified by the annals of antiquity. Rivers which were frozen in the times of Julius Cæsar and Augustus, the Tiber and the Rhone, flow now in the severest winters. The Black Sea is no more the hard frozen sea of Constantine Copronomus, and Germany and France are tropical regions in comparison with the degree of cold which the ancients ascribed to them. In comparison with such changes, the following may hardly seem worth noticing, to individuals who are not aware of the importance of even the smallest quantity in mathematical inquiries. According to Carlini, observations made at Milan indicate a diminution of the pressure of the atmosphere. The mean height of the barometer was

in. lin.
From 1764 to 1792 = 27—9·104
From 1792 to 1801 = 27—8·522

Difference ·582

Before I conclude, Gentlemen, this very imperfect attempt to show you rather my respect and gratitude, than to give you any information on the subjects which I have mentioned, I wish to acknowledge the satisfaction I derive from a comparison of the results of my observations, with some of the data, which were so ably presented to us in our last meeting.

My observations comprehend the interval already mentioned from the 17th of April 1823 to the 18th of April 1824. The mean heat deduced from them is 58·5° Fahr. for the whole year, 67·5° for the summer, and 47·2° for the winter. Mr Little indicates, in round numbers, 56° as the mean heat of the year. Besides that my observations did not begin with the 1st of January 1823, nor end on the 31st December of the same year, but were extended beyond the subsequent vernal equinox, a difference of 2·5° Fahr. is in the limits of the differences that may be ascribed to a difference in the method of observing, and the construction of the instruments. The mean result of Mr Little's observations for the last 3 years is 56·3°. Mr Josiah

Meigs rates the mean temperature of 1821 at 55·02°. The result I have presented it 2·7° greater than the mean heat M. de Humboldt ascribes to New York.

In his interesting dissertation on the *distribution of heat on the surface of the globe*, he observes, that the mean heat, during the month of October, can, under high northern latitudes, be considered as approaching the mean heat of the whole year. During the month of October, I found it to be 13·3° Cent. (55·9° Fahr.). The difference between the average heat of that month, and the result of all my observations for a year, was consequently but 1·4° Cent. (2·5° Fahr.). With respect to New York, M. de Humboldt rates the difference only 0·4° Cent. Our colleague states the mean temperature of October 1823 to be 54°, and the mean temperature of the whole year, 56°, difference 2°. With respect to 1824, the difference it only 1°, but during 1825 it amounted to 3°.

The *Isothermal* line of 10°, according to the ingenious system of the great German traveller, passes through this country, in 42° 45′ lat. one degree south of Dublin, half a degree north of Paris, 0° 5′ south of Prague, 1° 5′ North of Buda, and 2° 45′ north of Pekin. How astonishing must be, to persons unacquainted with this subject, such a similarity of temperature between Philadelphia, the latitude of which is 39° 56′, and Dublin, which situated in 51° 25′, and the difference of 5° Cent. (9° Fahr.) in the mean temperature of New York and Naples, though they are situated under the same latitude.

The changes of temperature are rapid in this country, and very great in comparison with the variations under the torrid zone. But it must not be forgotten that in Paris they amount to 44°, and in more northern countries of Europe to even 60°.

Gentlemen, researches in the field of science become daily more numerous, and unexpected results are the reward of indefatigable inquiry. Mayer is no more an authority. His speculations are superseded by experiments and analyses. The mean temperature of the Pole can be no more supposed to be zero, as that celebrated astronomer had stated. Arago, in discussing both hypotheses, viz. that this continent extends itself to the northern pole contiguously or by means of an Archipelago of islands, little separated from each other, and that the ocean surrounds that extremity of the axis of the earth, and availing himself of observations made from the 54° to the Melville islands, situated in 75° lat. has established, with great plausibility, that the temperature of the pole is 25° Cent.

Nay the heat of the human body has found an ingenious observer. It was known that in England, the temperature of our species is 36·7° Cent. (98.1° Fahr.). Dr John Davy, in his *account of the interior* of Ceylon, has concluded from observations made with several individuals, natives of Europe and of Ceylon, that by going from a cold or temperate climate to tropical countries, the heat of man increases 1°, and in some persons 1·5° Cent.

The country to which science has been indebted for Franklin and Rumford, the country which was among the first in furnishing data for the exact determination of the figure of the earth, cannot remain indifferent to exertions for the advancement of human knowledge.

General Intelligence.

Curious Change of Colour in Oxides of Cobalt and Zinc.—It has been observed, that when a mixed solution of zinc and of cobalt is precipitated, so as to furnish a mixture of the oxides, and this mixture is well washed and dried, it forms a white powder, which, when heated in a close vessel, loses water, and becomes of a beautiful green colour, though the oxide of cobalt does not amount to above one sixth part of the whole. When well prepared, it is probable that this compound may be found to be a very useful pigment.—*Quart. Jour.* 38.

Refrigerating Salt.—If we mix 57 parts of muriate of potash, with 32 of muriate of ammonia, and 10 of nitrate of potash, a refrigerating salt will be produced. This salt, put into four parts of water, and quickly agitated, will make the thermometer descend from 20° to 5° below zero, in Reaumur's thermometer.—Vauquelin, *Journal de Pharmacie.*

Iodine in Mineral Waters.—Iodine was first discovered in mineral waters by M. Angelini, who found it in the salt water of Voghera, and in the water of Sales in the Voguerais. M. Cantu, Professor of Chemistry at Turin, surprised at the wonderful effects of the sulphurous water of Castel Nova d'Asti, in the treatment of goitres, and other glandular maladies, examined it chemically, and found it more rich in iodine than any other.—*Mem. de Torino*, tom. xxix. p. 221.

Remarkable Hailstones with pyritic Nuclei.—The following very curious account of these hailstones is given in a letter from

Professor John of Berlin, to Baron Ferussac. It forms also part of a letter from Dr Eversmann, known by his travels in Asia, dated 16th September, from Orenburg, where he has landed property.

"Some days before our arrival at Sterlitamak, (more than 100 versts from Orenburg,) there arose a storm in which very remarkable hail fell. *The hailstones, which were tolerably large, contained a stoney and crystallized nucleus.* Thirty of them have been sent to our Governor, and I have received two specimens." They are of a brown colour like the auriferous pyrites of Beresowsky, in Siberia. Their surface is shrivelled and shining. The crystal forms a flattened octoëdron, whose edges are salient. The two diagonals of the base are five lines by four, and the distance of the summits is two lines. Sometimes the four angles of the base are truncated. *It seems that the constituent parts of these crystals are sulphur and metals.* No analysis of them has yet been made, but I shall perhaps have occasion to do this.—See *Bull. des. Sc. Phys.* Feb. 1825, p. 117.

Distance to which minutely-divided Matter may be carried by the Wind.—On the morning of the 19th of January last, Mr Forbes, on board the Clyde East Indiaman, bound to London, in lat. 10 deg. 40 min. N. and long. 27 deg. 41 min. W. and about 600 miles from the coast of Africa, was surprised to find the sails covered with a brownish sand, the particles of which, being examined by a microscope, appeared extremely minute. At 2 P. M. the same day, some of the sails being unbent, clouds of dust escaped from them, on their flapping against the masts. During the night, the wind had blowed fresh NE. by E. and the nearest land to windward was that of the African coast lying between Cape de Verd and the river Gambia. May not the seeds of many plants, found in remote and newly-formed islands, have been thus conveyed?—[*New Monthly Mag.*]

Variations in the Composition of the Atmosphere.—Mr Dalton states that he has found the oxygen in the atmosphere vary from 20·7 per cent. to 21·15 per cent. The latter was the case on the 8th of January last, the barometer being 30·9, wind NE. and very moderate, after three days of calm and gentle frost. The general state of the atmosphere yields only 20·7 or 20·8 per cent. of oxygen.—[*Ann. Phil.*]

Literary Property in France.—The king of France has appointed a commission to prepare a law for the protection of literary property. The members are, the Marquis of Lally—

Tollendal, Viscount Lainé, and Count Portalis, peers of France; Royer Collard, the Count de Montbron, and Pardessus, deputies; Bellart and de Vatimesnil, privy counsellors; Delaville and Villemain, magistrates; Auger, Raynouard, Andrieux, Parseval-Grandmaison, Picard, Al. Duval, Michaud, Dacier, Baron Cuvier, Baron Fourrier, and Quatremère de Quincy, members of the four Academies; and Baron Taylor, royal commissioner at the Théâtre-Français. To these are to be added Messrs Lemercier, Etienne, Moreau, and Champein; who, on the royal requisition, have been elected by the dramatic authors as delegates, to take a share in that part of the discussions of the commission which will relate to the interests of dramatic literature. At the first meeting of the commission it was unanimously resolved to invite the attendance of M. Talma, in consequence of the close connexion between the interests of dramatic authors, and those of actors. A long *projet* of a law has been submitted to the consideration of the commission, divided into twenty distinct heads; which, after they have been thoroughly discussed, will be formed into the four following chapters:—On the property of literary works in general; on the property of dramatic works; on the property of works of art; on the property of musical compositions.—[*New Monthly Mag.*]

Patents.—Two patents have recently been obtained, which promise very brilliant advantages to their respective proprietors, and a decided benefit to the public. The first is a French discovery of an extremely hard and beautiful cement or stucco, manufactured from lime and some other ingredients, by which every kind of marble may be imitated. It is as hard as marble, and resembles it closely. Such is its closeness of texture, that it will admit of a brick being struck against it till reduced to pieces, without its effecting the least scratch or indenture. Another admirable advantage is, that it admits of fresco painting, which will endure constant washing and cleaning without suffering any injury. Oil painting can be laid over the cement as well as water colours. Neither wet, heat, cold, nor frost, make any impression, nor do they, to all appearance, in the least disturb its particles; it will not flake off. The second patent is for ornaments of various kinds in *papier mache.* The specimens already produced are equal to the finest carvings; at present it is intended to supersede plaster ornaments for gilded frames, either of pictures or looking-glasses, and for embossed ornaments in panneling splendid rooms. If it be good for frames, it must also be good for friezes and cornices of rooms, halls, staircases, &c. Its peculiar advantages are, that it is extremely hard, very light, and cheap.—[*New Monthly Mag.*]

Mr Pond's Method of determining the Direction of the Meridian.—This method consists in placing two very distinct and well defined objects nearly at equal distances on each side of the north meridian line, and at a distance from each other nearly equal to twice the greatest elongation of the pole star. These objects are placed by means of a telescope, fixed like a transit on a horizontal axis, and pointed to the pole star, at its greatest elongation, and then at its reflected image, the central wire, which must necessarily describe a strictly vertical circle, will pass over some terrestrial object, which, if well defined, will serve for the mark; but if not, a mark must be set up so as to be bisected by the wire. The same being on the other side of the north meridian, the distances of the marks from the central wire must be accurately measured with a micrometer. The horizontal angles between the marks being measured by a theodolite, a meridian mark is to be erected exactly between them.—[*Brewster's Journal.*]

Remarkable Effect on the Cambridge Transit Instrument.—In adjusting the Transit Instrument recently put in the New Observatory at Cambridge, Mr Woodhouse observed, that the line of collimation deviated occasionally to the east or west of the meridian mark without any visible cause. He at last found that this effect arose from the approach of the assistant's body to the lateral braces used to steady the instrument in an invariable position at right angles to its axis. The expansion of the brace nearest him thrust the axis of the telescope aside. Mr Woodhouse proved that this was the cause of the deviation, by producing the same effect with hot cloths wrapped round the alternate braces, and he, therefore, provided a proper apparatus to protect the braces from the solar heat, during the approach of the sun to the meridian.—[*Brewster's Journal.*]

Committee for the Improvement of Glass for Optical Purposes.—We understand that the Royal Society, of London, and the Board of Longitude, are taking effectual measures for the improvement of Glass for optical purposes. A regular glasshouse has been built for this purpose, and a series of experiments will be immediately begun, under the direction of a committee of members of the Royal Society, from whose talents and diligence we anticipate the happiest results.—[*Brewster's Journal.*]

Electricity of Flame.—M. Pouillet has shown that, in a vertical flame formed by the combination of hydrogen with oxygen, the visible part of the flame, and the part without it, to the distance of a centimetre, is positively electrified, while, in the interior of the flame, resinous electricity alone is found.—*Ferussac's Bulletin.*]

Huygens' Mathematical Manuscripts.—A part of these manuscripts contain his correspondence with the philosophers of his time. It is to be regretted that he preserved only those which were the most interesting to him. Among them are his correspondence with Leibnitz and l'Hôpital, entire. Another part of these manuscripts consisting of several volumes written by the hand of Huygens, contain essays on several of his theories. The manuscripts of his published works have likewise been found. Joined to his dissertation on the percussion of bodies, are several pieces, to show, as it appears, the periods when his researches on this subject were made.

M. Uylenbrock, lecturer in the University of Leyden, proposes to publish the correspondence with Leibnitz and l'Hôpital, which is upon the differential calculus; in which Leibnitz explains the use of the calculus, to Huygens, and l'Hôpital solves several problems that the latter had proposed to him. The editor confining himself to the pieces of great interest, believes that he shall be able to reduce the work to 3 vols. 4to, and perhaps even to one.—[*Ferussac's Bulletin.*]

Light produced during Crystallization.—Dobereiner states, that a splendid instance of light produced during crystallization was observed by M. Buchner, of Magonza, during the sublimation of benzoic acid, previously mixed with pulverized carbon. The sublimation was carried on in a tall glass cylinder upon a stove, and when it had well begun there appeared an uninterrupted succession of sparks, continuing for half an hour, and which ceased only when the cylinder was removed from the stove.

Dobereiner states, that he has reason to believe those salts, containing no water of crystallization, are especially powerful in producing light during their crystallization.—*Gior. di Fisica*, vii, 470.

Test of the Presence of Muriatic or Nitric Acid, or Salts of these Acids.—For Muriatic Acid, or Muriates. Put pure nitric acid into a porcelain capsule, and throw to the bottom of it a very minute quantity of finely-divided gold, precipitated from its solution by sulphate of iron; put into the acid a minute fragment of any muriate, by degrees the gold will be surrounded by a light yellow tint, a certain sign that it is attacked. A fiftieth of a grain of a muriate may be detected in this manner.

For Nitric Acid or Nitrates. Operating in an inverse manner, the presence of a nitrate in a soluble salt may be ascertained. The gold should be put into muriatic acid as pure and as colourless as possible, and then a fragment of the salt, supposed to contain a nitrate, added; sometimes this experiment requires many hours for its completion, if there be but a small quantity of nitrate present.

This variation of Dr Wollaston's test, may at times be usefully employed.—[*Ann. de Chimic.* xxviii, 36.]

Meteor.—A brilliant meteor was observed on the evening of March 31st. about 8 o'clock, in the SSW.; its elevation when first observed being apparently 40°. A strong stream of light was thrown out in the direction from which it descended. Before it descended, the light disappeared entirely. From the appearances, it is probable that an aerolite (or meteoric stone) fell somewhere in this vicinity—Cambridge. It appears by an account from New Haven, that the same meteor was seen at that place, passing in a westerly direction. Its passage was accompanied with an explosion.—[W].

Celestial Phenomena for the months of April and May 1826, *computed for Cambridge, in lat.* 42° 23′ 28″ *N. and long.* 71° 7′ 25″ *W.* Mean time. Communicated by R. T. Paine, Esq.

April 1826.

D.	H.	M.	S.		
3	9	17	28	P. M.	☌ ☽ and Situla, in the ecliptic.
4					Greatest elong of ☿ east of ☉.
5	2	57	40	A. M.	Em. ♃ 1st. Sat.
6	8	37	07	P. M.	Em. ♃ 2d Sat.
"	9	26	14	"	Em. ♃ 1st. Sat.
12	5	5	16	"	✱ ☌ ☽ and ζ ♉.
13	4	43	19	"	✱ ☌ ☽ and ν ♊.
"	11	14	16	"	Em. ♃ 2d. Sat,
"	11	20	35	"	Em, ♃ 1st. Sat.
14	7	18		A. M.	□ of ♅ W. of ☉.
16	6	28	38	P. M.	☌ ☽ and ♓ , in the ecliptic, at nearest ap. ✱ 3′ ap. south of ☽.

D.	H.	M.	S.		
18	7	55	33	"	✶ ☌ ☽ and 55 ♌ in the ecliptic.
"	7	32	22	"	Em. of 55 ♌ 1′, 3 s. of ☽'s centre.
20	10	57	32	A. M.	☉ enters ♉.
21	1	15	01	"	Em. ♃ 1st. Sat.
"	1	51	31	"	Em. ♃ 2d. Sat.
22	9	36	59	P. M.	Em. ♃ 3d. Sat.
23	5	25	27	A. M.	☌ ☽ and ι ♎ in the ecliptic.
24	9	28		"	Inf. ☌ of ☿.
29	9	38	09	P. M.	Em. ♃ 1st Sat.
"	10	20	21	"	Em ♃ 2d. Sat.
30	1	35	29	A. M.	Em. ♃ 2d. Sat.

May 1826.

D.	H.	M.	S.		
1	2	59	08	A. M.	☌ ☽ and situla, a very near approach.
2	5	27	01	"	☌ ☽ and κ ♓, a near approach.
5	2	13		"	☍ of ♂.
6	11	32	43	P. M.	Em. ♃ 1st Sat.
8	8	24	12	"	Em. ♃ 2d. Sat.
9	10	57	27	"	☌ ☽ and ζ [illegible].
10	10	39	58	"	☌ ☽ and ν [illegible].
13	1	51	13	P. M.	✶ ☌ ☽ and A ♋.
"	7	55	35	"	☌ ☽ and 1 α ♋ in the ecliptic.
"	8	0	02	"	Immersion 1·5′ } north of ☽'s
"	9	7	49	"	Emersion 6·9 } centre.
"	8	49	47	"	☌ ☽ and Acubens.
14	1	27	20	A. M.	Em. ♃ 1st. Sat.
15	11	01	28	P. M.	Em. ♃ 2d. Sat.
16	8	42	31	"	☌ ☽ and e [illegible] in the ecliptic.
"	7	49	37	"	Immersion 3·2′ south } of ☽'s
"	9	5	59	"	Emersion 1·1′ north } centre.
21					☽ tot. ecl. invisible.
"	11	19	3	A. M.	☉ enters ♊.
22	2	1			Great. elong. of ☿ W. of ☉.
"	9	50	39	P. M.	Em. ♃ 1st. Sat.
27	4	06		A. M.	□ of ♃ E. of ☉
29	11	45	19	P. M.	Em. ♃ 1st Sat.

(To be continued.)

ERRATA.

In the notice of the comet, page 101, for 51° 32′ read 56° 32′. In the celestial phenomena, page 102, Nov. 26th. for 5 h. 41 m. read 5 h. 47 m.; and in the second line of the note at bottom of same page, *dele* ";" after "sunset."

ART. XXXII.—*A Review of some leading Points in the Official Character and Proceedings of the late President of the Royal Society.* By a Correspondent. [*Phil. Mag.*]

[The writer of the following Review relies upon the established liberality and candour of the Editor of the Philosophical Magazine, when he transmits for insertion in that valuable publication an article which may, probably, run counter to the usual train of his own sentiments and feelings. The main object of the writer is to enable the members of one of the most honourable of British Institutions, by a calm retrospect of past occurrences to diminish the evil effects which have resulted from them, as well as to prevent the recurrence of similar practical errors in future. He has advanced nothing as facts, but what he has carefully verified; and as he wishes those facts alone to make their due impression, he does not think it necessary to accompany them with his name.]

THE Royal Society, as Chamberlayne remarks, "chose for its motto *Nullius in verba*, to testify their resolution not to be enslaved by any of the greatest authority in their inquiries after nature;" and so long as their Presidents were changed with moderate frequency, and no one acquired any more authority or influence than was due to his talents and his virtues, independently of his rank (whatever that might be,) all continued to go on well. The arts and sciences, in their numerous departments, were promoted by the labours and inquiries of the different members of the Society; each brought from his own stock to deposite in the general storehouse; all was harmony; and bickering and usurpation were alike unknown. The distinctions which prevail in human society were not *forgotten*; but they were not permitted to operate injuriously in a society where all were, by its original constitution, FELLOWS. An authorized list of the members of the Royal Society circulated in 1693, only thirty years after its incorporation by charter, terminates thus:—"The reader may perceive by this list, how many sober, learned, solid, ingenious persons, of different *degrees*, *religions*, *countries*, *professions*, *trades*, and *fortunes*, have united and conspired, *laying aside all names of distinction*, amicably to promote experimental knowledge."

Indeed, it is only by determining thus to "lay aside all distinctions," except those which talents and genius confer, that a Society formed for the purpose of augmenting the sphere of natural knowledge in all its branches can be adequately efficient; for if it be "with wise intent" that

> "The hand of Nature on peculiar minds
> Imprints a different bias, and to each
> Decrees its province in the common toil,"

it is surely wise for such an institution to collect, arrange, and classify the results of the individual energies of its members, however diversified their several pursuits, or however varied the stations in political society which they occupy. Thus has the Royal Society proceeded in different periods of its history. It did not expel *Isaac Newton* at a time when he was too poor to defray the weekly charges of the Society; nor did it refuse to admit *Edmund Stone* or *Thomas Simpson*, or *James Ferguson*, although one had been *a gardener's son*, the other *a weaver*, and the third *a shepherd*.

These, and other important benefits, likely to accrue from the voluntary association of men of science, may undoubtedly be preserved, although any one of their number chosen to be their President should continue such for a series of years, or although he be a man of elevated or noble rank. The history of the Royal Society presents instances of this kind; as will be evident from the subjoined list of Presidents from the commencement of the Society to the present time.* But, in order that benefits like these may continue to result, be it recollected, as has always been observed, and will doubtless in future be found, that the Presidents of the Royal Society who most successfully promote its interests, are men ardently attached to some one branch of science, yet not depreciators of other departments of human research, men of candour, men free from the love of political intrigue, and free from its usual associate—the love of domination.

It will appear evident, then, without further preliminary observation, that the character, disposition, and talents, of a President

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* Presidents of the Royal Society from its origin.

	Elected.	*years in office.*
Lord Vicount Brouncker	April 1663	14
Sir Joseph Williamson, Knt.	Nov. 1677	3
Sir Christopher Wren, Knt.	Nov. 1680	2
Sir John Hoskins, Bart.	Nov. 1682	1
Sir Cyril Wyche, Bart.	Nov. 1683	1
Samuel Pepys, Esq.	Dec. 1684	2
Earl of Carbery	Nov. 1686	3
Earl of Pembroke and Montgomery	Nov. 1689	1
Sir Robert Southwell, Knt.	Dec. 1690	5
Earl of Halifax (Charles Montague, Esq.)	Nov. 1695	3
Lord Somers	Nov. 1698	5
Sir Isaac Newton, Knt.	Nov. 1703	24
Sir Hans Sloane, Bart.	Nov. 1727	14
Martin Folkes, Esq.	Nov. 1744	11
Earl of Macclesfield	Nov. 1752	12
Earl of Morton	Nov. 1764	4
James Burrow, Esq.	Sept. 1768	
James West, Esq.	Nov. 1768	4
James Burrow, Esq.	July 1772	
Sir John Pringle, Bart.	Nov. 1772	6
Mr., afterwards Sir Joseph Banks, Bart.	Nov. 1778	41

of a literary or a scientific society, will have an influence upon its members, its proceedings, and its utility, bearing some natural proportion to the interval during which he presides over it. Consequently, since the late Sir Joseph Banks occupied the chair of the President in the Royal Society for more than forty years, at an age of the world when science in almost every department and in every country of Europe was making the most rapid advances, it will become the duty of the impartial historian of British science to ascertain what were the qualifications of this gentleman to preside for so many years over that illustrious body, what were the topics of inquiry which he most encouraged, what were those which he uniformly repressed, and what have been the consequences with regard to certain sciences of Britain, in comparison with the cultivation and augmentation of the same in other parts of the world.

The following pages may probably assist in this inquiry: and I would simply premise, that though I shall throughout employ the language of frankness, as best calculated to elicit and exhibit truth, yet I have not the remotest inclination to violate the laws of propriety and decorum, or to lose sight of the solemn consideration that the subject of animadversion is now alike indifferent to human praise and blame. So far as my judgment and information will enable me, I shall represent things as they were; so that while on the one hand I shall "nothing extenuate," I should be equally resolved "to set down nought in malice," even (which however is not the case) if my personal intercourse with Sir Joseph had called into exercise that baneful passion.

Several of the eulogists of the late President have fancied that they could render his merits more prominent by placing them in contrast with those of his immediate predecessor, Sir John Pringle. I shall therefore be the more readily pardoned for adopting a like proceeding in this review.

Sir John Pringle was elected a fellow of the Royal Society in the year 1745, and had even then a high reputation for medical knowledge and skill. Afterwards he wrote pretty copiously upon many subjects connected with his profession, and communicated several interesting papers to the Transactions of the Royal Society; in this manner, as well as in consequence of an extensive practice, becoming very eminent both as a practical physician, and as a medical writer. But his reputation, exalted as it was in these respects, was not confined to them. He had a great love for science generally, and he cultivated it with corresponding ardour. Early in life he had read the works of Bacon with great attention, and his mind became in consequence predisposed to the genuine mode of philosophizing by means of well conducted experiments: he never suffered himself to be seduced by mere theory, but most valued and most promoted those sciences which rested on the firm basis of fact. With the exception that he had no relish for poetry, he had a well formed taste; and he was a man of extensive reading and of deep reflection. He was not too much of a philosopher to be ashamed of avowing his belief in a divine revelation; but read and thought much on the momentous subject of religion. He maintained for some time an active correspondence with the celebrated *Michaelis*, who addressed to him some valuable letters, in Latin, on Daniel's Prophecy of *the seventy weeks*, which Sir John published in 1773.

During the six years that Sir John had the honour of being President of the Royal Society, he adopted the practice of delivering an oration on the assignment of Sir Godfrey Copely's medal to the author of some valuable invention or discovery. He was led to this almost entirely by accident; but the addresses thus delivered, being intended to point out what was actually due to the individual who received the medal, by showing what had been effected before in the same department of research, became exceedingly valuable as brief historical disquisitions; and being each directed to a different topic of inquiry, they evinced such an extent and variety of reading, such a correctness of judgment, and such a freedom from bias or partiality, as were at once honourable to him, and to the society who had elected such a President. Of these discourses the 1st was "On the different kinds of air," delivered November 30, 1773, on the assignment of the Copleian medal to Dr *Priestley*; the 2d, "On the Torpedo," in 1774, on presenting the medal to Mr *Walsh*; the 3d, "On the attraction of mountains," in 1775, on presenting the medal to Dr *Maskelyne* for his observations at Schehallien; the 4th, "On preserving the health of mariners," delivered in 1776, on assigning the medal to Captain *Cook*; the 5th, "On the invention and improvements of the reflecting telescope," in 1777, on assigning the medal to Dr *Mudge* of Plymouth; the 6th and last, "On the theory of gunnery," was delivered on the day of his resignation, when he presented the medal in the name of the Society to Dr *Hutton* of Woolwich, on account of his important experiments on that subject.

Diversified as were the topics of these discourses, their author seems "at home" in each. His researches were often erudite; his remarks ingenious and solid, sometimes profound; his language elegant and perspicuous, occasionally passing into a stream of genuine eloquence which really enchants and captivates the reader.

Sir John was a man not merely of scientific, but of high moral character. He was of cheerful habits, but an enemy to all kinds of intemperance. His manners were kind, respectful, and obliging; but, says one of his biographers, "his sense of integrity and dignity would not permit him to adopt that false and superficial politeness, which treats all men alike, though ever so different in point of real estimation and merit, with the same show of cordiality and kindness."

Such was Sir John Pringle. Let me now attempt to delineate the character of his successor.

Sir **Joseph Banks** (born 1743, elevated to the rank of baronet in 1781,) was a man of good fortune, and is said to have received a liberal education, partly at Oxford. He early evinced an attachment to the pursuit of natural history, and in 1766, at twenty-three years of age, became a fellow of the Royal Society. In 1768 he set sail with Cook in the Endeavour, and during the whole of that interesting voyage paid considerable attention to the natural productions of the various parts of the world they visited. He was assisted in his zoological and botanical researches by Dr Solander, a pupil of Linnæus. I am not minutely acquainted with the nature and extent of the benefits mutually received and communicated by these two celebrated men; but one of the wicked wits of the day, who affected to be in the secret, attempted to develope it in a single couplet:

> "Through east, or west, or north, or south, they wander,
> You 'll find on shallow *Banks* feeds fat *Solander.*"

After the return from Cook's first voyage, Mr Banks made considerable preparations to accompany him a second time; but the circumnavigator and the naturalist had agreed so ill while they were together in the Endeavour, and Cook had been so thoroughly disgusted with the assumption of the great man and the unaccommodating airs of his companion, that he took effectual measures to free himself from like vexations during his second voyage. Mr Banks, to hide from the world his chagrin and mortification, and to appropriate to some useful purpose the expensive apparatus he had prepared to accompany Captain Cook, projected a voyage to Iceland: soon hiring a vessel, he was again accompanied by Dr Solander. Sir Joseph's biographer in the paper called The *New Times*, says on this occasion, "His hazards were rewarded by the *discovery* of the cave of Staffa." What was the nature of this *discovery* I cannot conjecture. Staffa had been then long known, and even described, though slightly, by Buchanan. Von Troil, Banks, and Solander, were conducted to Staffa by Mr Maclean, a Scotch gentleman of fortune, who had often been there before, and enabled our voyagers to *discover* precisely what he showed them.

Almost immediately after Mr Banks's return from this northern voyage, he began to take an active part in some of the measures then carrying on in the Royal Society; and on the resignation of Sir John Pringle, in November 1778, he was appointed to succeed him. The world began anxiously to inquire what where his requisites for this exalted station; but did not then receive a very satisfactory answer. He was known to be a man of enterprise and of strong passions; a warm friend while his friends were subservient to his purposes, and, if otherwise, what Dr Johnson denominated "a good hater." He was notoriously fond of farming, fond of grazing, fond of gardening, fond of "damming and sinking,"* and fond of domineering: these, however, were qualifications for the office so dubious that the public naturally sought for something more. What, they asked, has he published? Where are the *volumes* that bear his name? When they were answered "*Nowhere*," they asked again, What are his *pamphlets*, and on what subjects? Where are his papers in the Philosophical Transactions of the Society over which you have appointed him to preside, and on what do they treat? To these and similar questions no answer could then be returned: and if such inquiries were *now* to be proposed, his friends would have little else to say, except they felt inclined to exult in his little Essay on *blight*, and perhaps a diminutive disquisition or two on the manufacture of gooseberry-wine, or something like it, in the Horticultural Transactions.

Indeed, during the whole course of his long presidency he evinced an absolute ignorance of several of the most interesting and useful sciences. Of mathematics, either pure or mixed, he knew nothing. The sublime investigations of Landen, Euler, Lagrange, and Laplace, had no more charms for Sir Joseph, than for the rudest peasant that laboured on his Lincolnshire estates. Nor was he merely ignorant of these sciences. He had a *dislike* to them; and for many years indicated this dislike by some waspish and petulant expression from the chair whenever a mathematical paper was read. Up to more than forty years of age, I am positively assured that he knew scarcely any thing of chemistry; but in this department of knowledge, it was afterwards said, he made a respectable proficiency. Natural history has been generally acknowledged to be the only study which he pursued with ardour and relish; yet even here, if I am correctly informed, he made no eminent attainments. A friend of mine had an opportunity, a few years ago, of ascertaining the opinion of a very competent judge, one of the most eminent members of the Linnæan Society, as to this point. The following is an account of what passed between them.

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* This strange phrase was one which Sir Joseph delighted to give in the shape of a toast, among the Lincolnshire farmers. "Success to damming and sinking," meant success to draining the fens; but then it was delivered in an enigmatic approximation to profanity, which he thought he might venture upon without losing his character as a gentleman and a philosopher.

Q. Will you allow me, Sir, to ask what is your opinion of Sir Joseph Banks as a man of science?

A. I should conceive, Sir, there cannot be much need to ask such a question. You know he is called *the patron of science.*

Q. Yes, I know he is: but that does not prove that he *possesses* it. *I* have some doubts about the matter, and take the liberty to inquire of you, as one who knows him well. Is he really eminent as a natural historian?

A. He has a very extensive and valuable *library* in the department of natural history.

Q. So I have always understood: but pardon me, Sir, this does not meet my question. Allow me to ask again, Is he really eminent as a natural historian?

A. Natural history is a very comprehensive classification of knowledge; what department of it do you principally allude to?

Q. Really, Sir, I scarcely know how to direct my inquiries to a narrower point, as I am but little conversant with these matters. I have understood, however, that he is an eminent *botanist*: what is your opinion on that head?

A. Why, that if he be so reckoned, *it must have been in a company of washerwomen!*

Thus terminated the inquiry.

Well, but, say some, if Sir Joseph was not a man of profound knowledge in any one department, or of an excursive turn of mind which made him at least speciously acquainted with several, we presume he was a man of address, and probably one with some pretensions to eloquence. We presume he trod in the steps of his predecessor with regard to the anniversary oration on assigning the Copleian medal. No such thing. For some years Sir Joseph made no attempt of the kind: but it having been insinuated in the course of the discussions of which I shall presently have to speak, that he was incompetent to prepare and deliver a set discourse on any subject,—to put to silence these impudent calumniators, he delivered an address at the anniversary, November 30, 1784. In that year the medal was assigned to Dr Waring, for one of his papers *On the Summation of Series.* Of the address delivered on that occasion I have the happiness to possess a copy, probably the only one now in existence : to gratify the natural curiosity of the public on so interesting a matter, I here present the speech, retaining *bonâ fide*, the original orthography, punctuation, &c.

Sir Joseph Banks's Speech, November 30, 1784.*

[Exactly copied both as to orthography, punctuation, &c.]

Altho' your Council, whose Office it [is] to allott Sir Godfrey Copleys Annual Bounty to the Person by whose Communications the Progress of Science has received the most usefull Assistance ; observ'd with pleasure, that the Papers read at the meetings of this Year, which independant of Competition with each other, deserv'd that valuable testimony of the Societies approbation, were more numerous than usual ; they felt little hesitation in Forming their decission : remembering how materially Science had already Profited by D^r Waring's successfull Labors, considering the subject matter of his Essay as congenial to the Views of the Royal Society and above all admiring the able & Scientific manner in which he has treated that subject, more abstruse & complicated perhaps, Than any other in which the institution of the Royal Society interests itself. They were enabled to decide with Speed & perfect unanimity.

To Edward Waring then D^r of Physic Lucasian Professor of the Mathematic's in the University of Cambridge Fellow of the Royal Society of London & Member of the Academy of Sciences at Bologna I am directed to deliver that tribute which the Royal Society annually pays to the most approved merit, for his Paper entitled—on the Summation of Series, whose general term is a determinate Function of Z the distance from the first term of the Series.

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* I hold myself answerable for the authenticity of this curious document. No sooner had Sir Joseph terminated this address than a murmur of *Rigmarole! Rigmarole!* ran through " the faction," as they were termed. Some of the President's less judicious partisans immediately proposed the *publication* of his "admirable speech ;" but they were outvoted by such of his friends as were too wise to risk his reputation, and that of the society, on such a strange production. It was simply determined, therefore, that the President's copy of the *speech* should be lodged in the archives of the Society. On the succeeding day a friend of mine made faithfully and carefully the copy which I now possess. A few days afterwards other fellows of the Society visited the rooms in order to take copies ; but the document was removed, by the President or his friends, and has never since been seen.

To enter into a detail of the Merits of this valuable performance & explain to the learned Audience whom I have the honor of addressing the Various particulars in which our Author has excell'd his Cotemporaries, is a task, which the limited Bounds of my Talents directed as they have always been to the attainment of a different kind of knowledge will not enable me to undertake: The extent of Science is far beyond the grasp of an individual he who is ambitious to enlarge its bounds must to effect his purpose take his separate department and Finaly confine himself to that subject whatever it be in which fortunate application has enabled him to excell In truth I feel no humiliation when I declare that having dedicated my Youth to the Pursuit of another Science the Superficial knowledge of Mathematic's I have hitherto attaind howsoever it may enable me to distinguish conspicuous merit will not Empower me to enter into an explanatory detail of a work intended for the perusal of those only who are fully initiated in the deepest Mysteries of Mathematical knowledge by men whose acquirements have gain'd them reputation in that Line our Author is held in the highest esteem & his Works are by learned foreigners universaly deem'd equal at least to Those of his most admir'd Cotemporaries in all parts of Europe as is plainly evinc'd by the Controversy he in this very Paper maintains with some of the most respected literary Characters in Europe for the Priority of Publication of more than one of the Principal discoveries of the age we live in.

I consider it as fortunate for the Society & feel on that account no inconsiderable pleasure that we have it in our power to bear testimony to the merits of such an Essay so deeply learned & so ably written on a subject in which the Public had been told we were deficient delivered in to us at a time when an unfortunate dispute among the Members had robb'd our Meetings of the assistance of some of them whose literary abilities we cease not to respect how widely soever many of us may Differ from them in our judgements of the Matters which were then in Dispute among us The Nation whose eyes are ever intent on the Conduct of the Royal Society will be assur'd by this very Paper that even at the time when we were most Divided Mathematical Knowledge was to be Found among us & that the Society at all times & under all Circumstances are ready to reward it.

From the Appearance of our present Meeting I will venture to Foretell that our dissensions are at an end that the Gentlemen from whom I have had the misfortune to differ in opinion will abide by the decisions of the Society which they have repeatedly taken, agree with me in a Determination to throw a veil of Oblivion over all past animosities and unite once more in sincere Efforts towards the advancement of the Society the honor &

reputation of which we have all equaly pledg'd ourselves to support.

But enough of dissention a word never more I sincerely hope to be heard within these Walls dedicated as they are by a generous Monarch to the service of Science Peace & harmony should ever be found within them for under the influence of Peace & harmony among those who Profess to cultivate it Science can only flourish.

Let us unite once more then my Friends to Fulfil the wise purposes of our liberal Patron & Benefactor and resume at the same time the Prudent Conduct of our Predecessors who for more than a Century past supported the honor of this Society unsullied & have bequeath'd it to us pure as they received it they never Fail'd to sacrafice such resentment as arose among them to the good of the general Cause in which they felt themselves equaly embark'd for altho' some individuals among them have heretofore indulg'd their Feelings by appealing to the Public when they imagin'd the welfare of the Body at large was in danger they never once attempted to convert the Meetings instituted for the advancement of Knowledge into Assemblies of debate & Controversy.

[The President hereupon addressing Dr Shepherd, said]

Animated with the pleasing hope of returning peace let me now proceed to the most grateful part of the office I have the honor to hold under favor of your indulgence That of delivering to Merit the mark of regard which the Society has alotted for it's reward— to you then Dr Shepherd as the representative of Dr Waring I deliver this Medal requesting you to give it to him on the Earliest opportunity in your power when you present him, Sir with this token of the approbation of his Fellow labourers encourage him to persevere in unravelling the Clue of that difficult but justly admir'd Science in the annals of which he already holds so distinguish'd a place Assure him that the Royal Society ever anxious to discover & reward merit in whatever line it may be exerted never more fully Feel the pleasing Idea of fulfilling well the purposes of their institution than when they seize an opportunity to reward those who labour to extend the limits of that Science which enabled our illustrious Newton to penetrate into the deeper mysteries of nature and explain them to his fellow creatures in terms within the reach of their limited Comprehensions.

By this time the reader will probably inquire with eagerness, Through what strange train of circumstances could an individual so sadly disqualified be elevated to "the chair" of the Royal Society? This train, intricate and involved as it has usually been deemed, it will not now be difficult to explore.

Some of the most brilliant discoveries in electricity, were, as every one knows, made by the celebrated Dr FRANKLIN; and, at an age of the world when this country was agitated by all the trying events of a war with America, Dr Franklin had the misfortune to be *an American*. Among this philosopher's numerous happy applications of his electrical discoveries, was that of *elevated pointed conductors* to secure buildings from injury by lightning; an application which was warmly approved, and eagerly recommended by the most eminent electricians then living. In luckless hour, however, Mr *Benjamin Wilson* (the father, I believe, of the present Sir Robert Wilson, and at that time, or soon after, contractor for the *painting* under the Honourable Board of Ordnance) *objected* to the use of pointed conductors, recommending instead of them conductors with *knobs* at their superior extremeties. "It was by *his* obstinacy and improper conduct (says Dr Thomson*) that he introduced those unhappy divisions which had so unfortunate an effect upon the Royal Society, and were so disgraceful to science and philosophy." Disgraceful, indeed, they were, both on account of the temper with which they were conducted, and the incessant violation of the principles of true philosophy which occurred in the the writings of Mr Wilson and his adherents. Philosophers in other parts of Europe wondered what strange fatality could have fallen upon English men of science, that they could force this into a topic of controversy; and neither then nor since have they uttered a syllable in favour of blunt conductors.† The truth, however, is, that had it not been for the intermixture of political feeling with the principles of the discussion, it could not have been kept alive for a single month. The American war had been the occasion of scattering the seeds of political animosity far and wide; and, since Franklin was a politician as well as a philosopher, it was by no means difficult to insinuate that they who agreed with him in his philosophical speculations agreed with him, likewise, in his political creed. Thus, with many, the opinions of a philosopher as to the *blunts* and the *points*, were regarded as the index of his opinions as to the American war; and the celebrated dispute among the "*little-*" and the "*big-endians*," recorded by Lemuel Gulliver, furnished an apt representation of the folly and the rancour which found their way into this discussion.

Ere long, the Royal Patron of the Society, whose strong feeling in reference to the American war is well known, became interested in the controversy, and often gave unequivocal indications of the manner in which he wished to see it decided. This soon reduced it to neither more nor less than a government question. In 1773, when it was proposed to fix conductors to the powder magazine belonging to the Board of Ordnance at Purfleet, that Board applied to the Royal Society for their opinion as to the most proper kind to be employed. The Society replied by quoting their own annual advertisement from the year 1762 downward, "That it is an established rule of the Society, to which they will always adhere, *never to give their opinion, as a body*, on any subject either of nature or art, that comes before them." The Society were then requested to appoint a Committee for this purpose. After much discussion this was agreed to, and a Committee, consisting of Mr Cavendish, Dr Watson, Dr Franklin, Mr Robertson, and Mr Wilson, was appointed. After examining the building, the four gentlemen first named, recommended *pointed* conductors: Mr Wilson dissented from their judgment, and assigned his reasons in a long paper. His notions were refuted by Nairne, Henley, Swift, and others. Dr Musgrave, on the other hand, defended his speculations.

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* History of the Royal Society, p. 444.

† See Biot—*Traité de Physique*, tom. ii. pp. 442—460.

In 1777 the Purfleet magazine received damage from lightning, although it had been previously furnished with conductors. The Royal Society, again requested to give an opinion, appointed a Committee of nine of the most distinguished electricians: their deliberate judgment was again in favour of pointed conductors, and again was their judgment opposed by Mr Wilson. In this stage of the business the Royal Patron of the Society directed Sir John Pringle to employ his official influence in strengthening Mr Wilson's hands. Sir John replied, that "duty as well as inclination would always induce him to execute His Majesty's wishes to the utmost of his power: but, Sire, (said he) I *cannot reverse the laws and operations of nature.*" "Then," said His Majesty, "*perhaps, Sir John, you had better resign?*"* Sir John took the hint, and resigned at the next anniversary, Sir Joseph Banks being appointed his successor the same evening. Whether *he* had or had not engaged to *reverse* the laws of nature, I am not prepared to say.

Sir Joseph was no sooner seated in the President's chair than he began to manifest his dislike of Americans and American philosophy,* and of all those members who accidentally testified their esteem of his learned predecessor. He also gave the most decisive indications of his philosophical bigotry, of his determination unduly to exalt some branches of inquiry, and as unduly to depreciate others; and of another determination, which he had not sufficient discretion to disguise, to convert a fellowship or brotherhood of philosophers, into a *monarchy*, or rather into a *despotism*, of which *he* alone was to be the focus of power and authority. Such is the force of self-delusion, when a coterie of sycophantic danglers surround an individual of this description, and foster his love of domination, that it would seem as though Sir Joseph actually fancied himself a kind of monarch, and formed his phraseology and expected to be approached accordingly. It was no longer the Council of the Royal Society, or the Secretaries of that learned body, but "MY *Council*," "MY *Secretaries*," "MY *Assessors*," "MY *Society*," &c. He held *his court* in Soho Square; and none but those who were introduced into the regal apartments there in due form, and danced attendance with due frequency, could obtain admission into the Royal Society, or continue to attend its meetings with comfort, if they had been elected fellows in better days.

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* Soon after this occurrence a friend of Franklin wrote an epigram which may not be deemed unworthy of preservation here:

While you, great George, for knowledge hunt,
And sharp conductors change for blunt,
The nation's out of joint:
Franklin a wiser course pursues,
And all your thunder useless views,
By keeping to the point.

That men of real genius and science should be disgusted with all this, was naturally to be expected; as well as that men of independence should make some efforts to deliver themselves from so disgraceful a thraldom. Hence originated the new class of dissensions which agitated the Royal Society between the years 1781 and 1785, and to which the eulogists of Sir Joseph Banks have now so unwisely recalled the public attention. Of these dissensions it is the more necessary a correct account should here be exhibited; because some of Sir Joseph's partisans, as though the lapse of six-and-thirty years had not been sufficient to cool their resentment, make them the basis of recent and renewed calumnies.†

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* This anti-American spirit is scarcely yet extinguished. Seven years ago there were not more than *three* American fellows of the Royal Society; and even at the present moment there are not *six*.

† A biographer of Sir Joseph Banks in the *New Times* of July 14, 1820, whose ignorance of science and of facts is so obvious, that it would be a waste of time to render it more prominent, terminates his misrepresentation of these matters thus:

" All intellectual propensities have their *merits* [those of lying, slandering, and thieving for example], and the use of practical mathematics is important and extensive. We honour the great inventors—the world is debtor to NEWTON. But of a thousand mathematicians, not the human cube root has

"The bitter spirit," (as the writer in the New Times calls it) did not "break out on the dismissal of Dr Hutton from one of the secretaryships," but much earlier. Some of the causes which fomented it will appear by a few quotations from a pamphlet entitled "An History of the Instances of Exclusion from the Royal Society," published early in 1784.

"The charge we bring against Sir Joseph Banks is, that, though not entrusted with any such power, either by statute or custom, and very unfit, from his acknowledged violence of temper, and from his incapacity to judge of literary qualifications, in which he is himself shamefully deficient, to be entrusted with it, he has repeatedly interposed in a clandestine manner, to procure rejections of proper candidates, with the visible design of taking away the privilege of the body at large, and making himself the sole master of the admissions,—in other words, the *monarch* of the Society."

In proof of this charge, we are told that during the twelve weeks which, according to the statutes, the certificate recom-

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ever been, or will be, more than the depository of the dusty problems, that the bookmakers of the art, the Simpsons, and Huttons, and Bonnycastles, have transmitted to them. This pride 'that puffeth up,' has had more fatal powers of perversion, and religion has no where found more inveterate prejudice or more morbid repulsion than among those men, rendered incapable of discerning truth, unless it came in the whole dignity of an algebraic formula. The bitter spirit broke out in the Royal Society on the dismissal of Dr Hutton from one of the secretaryships. How Dr Hutton, whose life, till he was mature, was spent in keeping a village school in Westmoreland, [*videlicet*, the *village* of Newcastle upon Tyne,] could have sustained the office without numberless offences against the habits of good society, it is difficult to conjecture; and his merits as a mathematician were commonplace.

'Lands he could measure, terms and tides presage;
And even the saying ran, that he could gauge.'

"Sir Joseph Banks, in point of general accomplishment, public utility, and rational and enlarged employment of his understanding, was worth the whole host, of which no single name did honour even to their own narrow pursuit. Horsley, afterwards a bishop, was the principal among the disturbers. His Commentary on the *Principia*, the most meagre and inefficient that ever came from the press, is this man's tribute to science. But he was virulent, insolent, and intriguing. The Bench restrained him, and he gradually cooled, but in the hostility against Sir Joseph Banks he gave full way to the bitterness of his nature. The President's conduct was put to the vote, and on the 8th of January, 1784, the Resolution 'that this Society do approve of Sir Joseph Banks for their President, and will support him,' was carried by a great majority. Measures of conciliation were now adopted. A vote was passed, that Dr Hutton had done nothing to forfeit the confidence of the Society: and, on the other hand, that it would be more convenient to have his office executed by a resident in London. Since that period opposition has slept. The Presidency has been in honour and activity."

mending a candidate, hung up in the Society's room, it was the habit of Sir Joseph to prejudice the minds of those who attended the Soho Square *levees*, by making known his resolution in phraseology not very courtly, but suited to the purpose and varied to accord with the occasion. "We want no *mathematicians.*" "No more *worshippers of old Cardan* for me." "I 'll have no *schoolmasters.*" "Let us have no *country surgeons.*" "He! why he is an *author!* Who could think of proposing him? We want no *authors;*" and so on. If these, and similar remarks, scattered with great activity during the twelve weeks' probation, seemed likely to fail in their effect, then "the President would run about the room on a night of election, out of breath with anger and impatience, seducing the ignorant, awing the timid, and deceiving the wise; cajoling as many as possible to put in black-balls: and often "inducing the candidate, or his friends from an apprehension of rejection, to avoid the mortification by taking down the certificate."

Among the candidates rejected principally by black-balling, in the years 1781, 1782, and 1783, were, Mr Henry Clarke, of Manchester; Mr Meyrick (who was black-balled by the President asking more than one hundred persons in the room to vote against him, *an ascertained fact*); Dr Bates, a physician at Buckingham; Dr Hallifax; Dr Enfield (here the cry was, "I 'll have no *Dissenters*"); Dr Beerenbrock and Dr Blane, two eminent physicians; and Major Desbarres, the friend and maritime tutor of Captain Cook. Shortly after the black-balling of this latter named gentleman, the following paragraph appeared in the public papers:

"Yesterday Major Desbarres kissed His Majesty's hand on being appointed Governor of Nova Scotia. This reward, we hear, has been conferred on this able and spirited officer, for great national services, in recompense of much time and much money, for having saved, by his *philosophical* labours, many of the king's ships, and the lives of many of our fellow subjects."

The preceding list of exclusions serves to prove, that in the early portions of Sir Joseph's reign, his antipathy was not merely to mathematicians.

(*To be continued.*)

Art. XXXIII.—1. *Prof. Schumacker's Astronomische Abhandlungen.* Altona. 1823. *A Memoir on Refractive and Dispersive Powers.* By M. Frauenhofer.

2. *Transactions of the Royal Society, Edinburgh, Vol. IX. On a Monochromatic Lamp,* &c. By Dr Brewster. *On the Absorption of Light by coloured Media.* By J. F. W. Herschell, Esq.

3. *Some Account of the late M. Guinand, and his Improvements in the Manufacture of Glass.* 8vo. Lond. 1825. [British Critic.]

M. Frauenhofer has been long known on the continent as a very distinguished practical optician. He has succeeded beyond any artist in this country, in producing flint glass for optical purposes of the most complete transparency and freedom from flaws and defects. This superiority in his glass has enabled him to prosecute some very important researches, an account of which is contained in the memoir named at the head of this article, as inserted in French, in the well known journal of Prof. Schumacker.

His primary object was to determine with great exactness, for the formation of achromatic object glasses, the dispersive powers of different species of glass. He first tried the effect of correcting the colour by opposing prisms, viewed through a telescope, which is in fact the same method as that originally proposed by Dr Brewster. But it became an object of attention to examine the dispersion of each coloured ray separately. To do this is a problem which has always been attended with the essential difficulty of not being able to fix upon rays in the spectrum which are strictly homogeneous, and which can at all times be identified with certainty. In order to get over this difficulty, M. Frauenhofer tried, without success, different coloured media and flames; to trials of this kind we shall have occasion to allude in the sequel, as leading to some important discoveries. Our artist, however, next adopted a plan which he considered successful; this was to place six lamps in a row behind a small aperture, close before which was a prism. The separate spectra of each lamp were thus thrown, so that the prism under trial, which was placed at nearly seven hundred feet distance, received only the red rays (for example) from one lamp, and the blue from another, &c., by which means the colours appeared in the form of distinct spaces, separated entirely from each other. We cannot help feeling some difficulty as to the application of this method, but perhaps the description itself is not the clearest that might be given. We do not feel sure that the rays were strictly homogeneous; however, they were capable of exact indentification from this further contrivance; a narrow aperture was made in the screen above the six lamps, through which the light of another lamp passed, and was received on the second prism; in viewing this, a bright line was seen at the limits of the red and yellow spaces; this was exactly defined, and by means of its invariable position, in comparison with the coloured spaces below, the observer could always be assured that the same identical ray fell on his prism. A number of measurements were thus made with great exactness, from which the great differences in the ratios of refraction for the same ray in different media, are clearly ascertained.

But the most important point was the appearance of the bright line above mentioned. This M. Frauenhofer next proceeded to study; he found it exhibited alike by the light from all flames, &c., when received through a narrow aperture. He next tried the light of the sun; this was received into a dark room through a narrow crevice, at the distance of twenty-four feet, by a prism of excellent flint glass; in looking at the spectrum thus formed through a small telescope, he observed not only the bright line before spoken of, but an infinity of lines, some dark and some bright, crossing every part of the spectrum at right angles to the direction of its elongation, and not forming the boundaries of the different coloured spaces, but existing in the middle of them, and in fact, distributed in some places more plentifully than in others along the whole length, in some parts more conspicuous, and in others more faint. Of all these lines the observer has given an accurate delineation; he counted upon the whole 574 of them. If the aperture be so wide as to subtend an angle of more than 15″ to the eye, the lines disappear. Some of the fainter ones also are not seen, unless the eye be shaded from the glare of the brighter parts. With English flint glass, M. Frauenhofer could only see the brightest lines; but with every sort of glass of his own manufacture, and with prisms formed of liquids, they were all distinctly seen. He then proceeded by an extended series of measurements, with a repeating circle, to determine the angles of deviation which these lines formed when viewed through different media. These lines in fact supply the great desideratum in researches of this nature, and enabled him to determine the deviations belonging to points in the spectrum strictly definite, with any degree of accuracy.

From observing the great number of lines crossing the spectrum, we might be led to suppose that the inflection of light at the edges of the aperture had some connexion with the phenomenon; in order to examine this point, M. Frauenhofer varied the experiment in the following manner: He received the rays through a small circular hole nearly 15″ in diameter; the spectrum thus formed had almost no breadth, but in order to widen it, M. Frauenhofer made the rays pass through a semi-cylinder of glass. By this means the length and order of colours remained unaltered, but the breadth being magnified, he saw as before all the lines. By means of the same contrivance he detected similar lines in the light of the planet Venus, without employing any aperture; the brightest lines only were visible, but they coincided in position with the corresponding ones in the solar spectrum. The light of some of the principal fixed stars was subjected to the same examination; in some of these, lines were observed in positions different from those before observed. The electric light was tried in the same way; the points of two conductors were connected by a fine fibre of glass, along which the succession of sparks was so rapid as to produce the appearance of a fine line of light. In the spectrum formed by this light, (without passing any aperture,) lines different from any of the former were observed. The light of several was similarly examined, and several curious results obtained.

Such is a brief outline of the most important parts of M. Frauenhofer's experiments; they indicate a very remarkable property of light, and present appearances which we believe have not yet been accounted for on any known principles. We must here take occasion to remind our readers, that the discovery of the fact itself, (though evidently unknown to M. Frauenhofer,) was made some years ago by Dr Wollaston. His experiment was, however, somewhat different; and owing to the great superiority of his glass, M. Frauenhofer has the merit of having ascertained the almost infinite number of those lines, which in Dr Wollaston's experiments appeared only a few.

M. Frauenhofer must also have the credit of being the first to apply these lines to the purpose of accurate determination of the dispersive power, although Dr Wollaston made a few observations of this kind. It may be satisfactory to many of our readers if we here mention, that with an ordinary prism of English glass, the principal lines may be very well seen by looking through the prism at a narrow aperture in a shutter, or screen placed against a window so as to receive the light of the clouds: this was Dr Wollaston's method: his experiments are given in the "Phil. Trans." 1802; he examined also the light from flame. If any of our readers are inclined to try the experiment, we recommend particularly to them to look at the blue part of a candle flame through a narrow slit; the separation of the colours is very wide and complete.

The mere inspection of the prismatic colours is sufficient to show that the different parts of the spectrum, independently of their colour, possess very different degrees of brightness or illuminating intensity. The late Sir W. Herschell was, we believe, the first who attempted any accurate determination of these relative intensities; he found the greatest illumination in the yellowish green space, and a gradual decrease from thence towards each extremity. M. Frauenhofer tried similar experiments by a different method, and his determinations were made with greater attention to exactness than perhaps any former; but there appear to us two essential difficulties in his method. In the first place, the intensity of each coloured ray was to be equalised with the white or yellowish light reflected by a plane mirror from a lamp; M. Frauenhofer considers it easy, with a little practice, for the eye to judge of this equalisation with the requisite accuracy. This, we must confess, appears to us very doubtful; though the sensation of colour and of intensity may possibly depend on modifications of the same cause, yet the two sensations follow such very different laws, and that difference is dependent upon principles so wholly unknown to us, that we can hardly conceive the possibility of abstracting so entirely from the idea of colour that of intensity, as to enable the mind to decide in any thing like a certain and satisfactory manner, upon the equality of illuminating effect in lights of two different colours simultaneously presented to the eye.

Another and more serious difficulty appears to us to arise from the following considerations: supposing the illuminating intensities to be really equal, it is well established, that if two rays of light, one of a colour approaching more to whiteness than the other, be presented in juxta-position to the eye, the deeper colour of the one will be diluted by the proximity of the lighter colour of the other; that is to say, though not actually combined or blended together, the sensation which the one produces in the eye tends to diminish that which arises from the other. If this, as is highly probable, is owing to the different convergency required for the two, it will obviously take place in a greater degree, in proportion as the coloured ray differs in refrangibility from the white. (See *Edin. Phil. Journ.* No, 19, p. 33.

Whatever weight may be attributed to the objections against this particular method, it is certain that the illuminating intensity sustains a regular decrease from the central yellowish green to the violet on one side, and the red on the other. The series of numbers given by M. Frauenhofer decrease in a more rapid ratio than those found by any other observers, and the tendency of the causes just considered as influencing his results, would be precisely that of producing this rapid diminution. But the decrease of illuminating power towards the red boundary, will become a point of considerable interest in the sequel.

M. Frauenhofer's observations on the illuminating powers of the prismatic rays, led him to several suggestions of practical importance in the construction of telescopes. He attends particularly to the distinction between diminishing the aberration of colour, and producing greater distinctness in the image; as also to the aberration from the want of achromatism in the human eye. When different specimens of glass were examined by the accurate test of the spectral lines, the difference in their dispersive powers was shown, when not otherwise capable of detection. M. Frauenhofer found differences of this kind in specimens taken not only from the same crucible, but from the opposite parts of the same piece of glass. By unwearied diligence and laborious trials, he has, however, at length succeeded in the manufacture of flint glass, to such a degree, that in a crucible containing four hundred pounds, two pieces, one taken from the bottom and the other from the top of the same mass, exhibited absolutely the same power.

This becomes the place for noticing the results obtained by a fellow-labourer in the same work, M. Guinand. The small publication we have named relative to this individual, is one which we have perused with considerable interest, as exhibiting a remarkable instance of the power of intuitive mechanical skill, in surmounting all the obstacles which circumstances and situation conspired to place in the way of its developement. M. Guinand was the son of a joiner at Neufchatel; as a youth he worked at that trade; subsequently made watch cases; and thus acquiring some idea of casting metals, undertook, on examining a reflecting telescope, to make one; in which he soon succeeded, without any knowledge of optics, and left entirely to his own resources for every part of the work. His next attempt was to make a pair of spectacles. He learned the art of grinding and polishing the lenses by having once witnessed the process. He hence proceeded to make lenses of telescopes, and constructed several small refracting ones. He now accidentally became acquainted with the principle of the achromatic object glass; and all his energies and labours seemed concentrated upon the means of endeavouring to procure glass free from imperfections for this purpose. This is in fact one of the most difficult problems with which the practical optician is concerned: and the patience, the sagacity, the perseverance, which M. Guinand displayed, in a long series of attempts under the most discouraging circumstances, to obtain his object, were truly surprising. At every failure he seemed to be occupied solely in studying the cause which had occasioned it. And thus, step by step, he contrived to approach at length towards the wished for object, and produced glass more free from striæ and imperfections than any before made. Every disappointment taught him some further improvement, and it was thus that he acquired, what is perhaps the distinguishing characteristic of his method, the mode of joining together into one large disk separate pieces of glass, selected as the most perfectly homogeneous. These he contrived to soften and unite together again, after which they were formed into the required lens, without any perceptible joining, or imperfection: in this way he has formed lenses of twelve or eighteen inches diameter. In 1805, his fame had reached M. Frauenhofer, who invited him to Bavaria, to give his important services to the establishment of Benedictbauern, where glass for optical purposes is largely manufactured under M. Frauenhofer's direction. The glass made by M. Guinand has since become known over Europe; specimens have been tried by the opticians and astronomers of France and our own country. The report of that eminent artist, M. Tulley, as to its great superiority to any made in this country, is couched in the strongest terms; and there can be little doubt that owing to the very perfect transparency which it possesses, we may expect a great increase in the power of refracting telescopes, hitherto so much limited in their degree of improvement. M. Guinand returned to his native place, and continued the construction of telescopes with uncommon ingenuity and success, himself not only having melted, formed, and polished the glasses, and calculated the adjustments, but also constructed every part of the apparatus, and put it together. This remarkable example of untaught genius died in 1823, aged seventy-six. His secret is confined to his son, who undertakes to continue the manufacture so important to the scientific world, upon the same principles as his father.

We before mentioned that M. Frauenhofer's first attempts were directed to obtaining homogeneous light by means of flames and coloured media; in this he was unsuccessful. Dr Brewster, however, and Mr Herschel have been more fortunate. In the memoirs above named by these two distinguished individuals, a great number of experiments are detailed, having in many instances a similar object in view.

Dr Brewster was in want of homogeneous light, to illuminate objects under microscopic examination. Mr Herschel wished to obtain it for the prosecution of certain optical researches. Dr Brewster, after numerous trials, ascertained the remarkable fact, that almost all bodies in which the combustion is imperfect, such as paper, linen, *&c.* gave a light in which strictly homogenous yellow rays predominated; that the yellow light increased with the *humidity* of these bodies; and that a great proportion of the same light was generated when various flames were urged mechanically with a blowpipe, or a pair of bellows. He thence concludes, that the yellow rays are the produce of an imperfect combustion. However, the most important circumstance was that the presence of aqueous vapour increased the quantity of yellow light; this was a new fact, and supplied Dr Brewster with a lamp whose light was truly homogeneous. Diluted alcohol is the pabulum he employs, and he has suggested a convenient form for a lamp for the purpose wanted.

Various media, such as coloured glasses, were also tried. Dr Brewster investigated the effect of heat in changing the tints of these glasses; in some, the power of absorbing particular colours is altered transiently, in others permanently. He tried the effect of different media in absorbing the different rays of the spectrum, and has given delineations of the spectrum, as seen through different coloured glasses.

In Mr Hershel's experiments the object was nearly the same in the first instance, but he has pursued it in a somewhat different manner from Dr Brewster, and has arrived at some other results of considerable consequence.

He first examined, as also Dr Brewster did, the effects of certain coloured glasses in almost obliterating certain coloured spaces in the spectrum, whilst others were transmitted in all their brilliancy. This fact was noticed by Dr Young. Mr Herschel, in applying to the examination of it the uncommon powers of his analytical skill, has resolved the phenomena into their most general expression, and thus traced the causes of many interesting consequences which otherwise would not have been deduced.

For example, one of the glasses he tried was of a ruby red colour; this permitted to pass almost the whole red, and a considerable portion of the orange; and even in strong lights a portion of yellow or a trace of green, but the rest were obliterated. He represents the effect by conceiving a straight line divided according to the proportions of the coloured spaces, to be taken as the abscissa, and at each point ordinates erected, representing the proportion of rays transmitted by any medium; the extremities of these ordinates give a curve, which he calls *the type* of this medium. The nature of this curve is determined by observation for each medium; but Mr Hershell has given an analytical expression, showing the law by which the nature of the curve is altered, according to an increase of thickness in the medium; this is in fact one of the most curious parts of the subject.

"It would appear at first sight," Mr Herschel observes, "that the effect of doubling or tripling the thickness of any coloured medium, would simply be to increase the depth and intensity of the tint, but not to alter its character. If a white object appear blue through a blue glass, we should expect it to appear still bluer through two, and yet more so through three such glasses. The above formula shows, however, that this is so far from being the case, that the tint of the emergent pencil is essentially dependent on the thickness of the medium; and that it is only from a knowledge of the relative values of the ratios of the intensity after traversing a thickness equal to unity, for the various parts of the spectrum, that we can say *à priori*, whether the tint of a thick glass will retain any similarity to that of a thin one of the same kind." (p. 447.)

The fact is, the quantity of any coloured ray, transmitted by an homogeneous medium, decreases in *geometrical* progression, as the thickness increases in *arithmetical*. Thus however trifling the difference may be at first in the effect of two media, it is always possible to render it sensible by taking a sufficiently great thickness; thus the water of the lake of Geneva is indigo-blue, that of the lake of Como, emerald-green, when viewed through a considerable thickness, though colourless in small quantities. Of this, numerous other instances will occur; such as the difference in the colour of the sea according to its depth, so well known to pilots, as often enabling them to perceive their approach to shoals, &c.

"In some instances the curve has two unequal maxima in different parts of the spectrum; and if at the same time the greater of these should happen to correspond to a ray of feebler illuminating power than the less, the tint, in small thicknesses of the medium, will, generally speaking, be that of the lesser maximum; the greater vividness of these rays giving them a predominance over the others, though more numerous; but as this inequality of number increases with the increase of thickness, the feebler rays will at length begin to influence the tint, and finally obtain the predominance: thus producing in several cases, a complete change of colour, not a little surprising to those who are ignorant of its cause. Dr Thomson's muriated liquor, (chloride of sulphur,) which is yellowish green in very small thicknesses, and bright red in considerable ones, is a case in point; a solution of sap-green presents the same phenomenon yet more strikingly. If enclosed between glass plates, slightly inclined, so as to form a thin wedge, its colour towards the edge will appear emerald green, and towards the back, blood red, passing in the intermediate thicknesses through a kind of livid neutral tint."

The existence of any real homogeneous yellow in the solar spectrum, has been denied by Dr Wollaston. The researches both of Mr Herschel and of Dr Brewster tend to show, that though the insulation of these rays in perfect purity may not be practicable, yet they may be so far separated, as to place their existence beyond all doubt. Dr Brewster considers these rays as encroaching on the limits both of the red and green. Mr Herschel attributes to them a breadth not less than one-fourth of the interval between red and blue. Dr Brewster draws the conclusion, that both the orange and green are really composite colours.

Of the numerous subsequent experiments of Mr Herschel, we shall not give any details; they are all of a very interesting nature, and exhibit several very singular changes in the aspect of the spectrum, by a mere addition of thickness in the coloured glasses: the curves which represent them, are all delineated.

From the solar light Mr Herschel afterwards turned his attention to that from flames, &c.: these lights differ extremely in their *types* when examined by the prism, and that in an apparently most capricious manner. Among other results, he found that sulphur, at a stage of inflammation which is extremely violent, as when thrown into a white hot crucible, emits a perfectly homogeneous and brilliant yellow light. In examining the light of a spirit lamp, it was found to become perfectly homogeneous when viewed through a glass consisting of a pale orange and a pale green one, cemented together. A lantern formed of such glass, would afford a monochromatic lamp for microscopical purposes.

Some beautiful appearances are detailed, as produced from the tinge given to flames by various substances held in solution with alcohol, &c.

One of the first glasses which Mr Herschel tried, and one which gave the most important results, was of that blue kind with a purplish tint which is employed for finger glasses, &c. When the spectrum is viewed through a thickness of .04 inch of this glass, the red space was divided into two by a dark line; other changes took place in the rest of the spectrum, which we need not here detail. With a double thickness of this glass further alterations were observed; among which was that the outer red alone remained visible, the inner being totally obliterated. When a great many thicknesses were laid together, the extreme red and violet only were transmitted. Mr Herschel directed his attention particularly to these outer red rays, and we extract the following important observations upon them:

"The species of light alluded to is remarkable; first, for its perfect homogeneity, and secondly, for its position in the spectrum. When the solar spectrum received on a white paper in a darkened room is viewed through a moderate thickness (.08 inch) of that glass, cemented to any red glass of a tolerably pure colour, it will be seen reduced to a perfectly circular and well defined image of a deep red colour. If a pin be now stuck in the *centre* of the red circle it will be found on removing the glass from the eye, to have been fixed in what an ordinary observer would call the very *furthest termination* of the red rays; and a mark similarly made at its circumference, will appear to lie wholly without the spectrum, among the dispersed light which usually hangs about its edges: in other words, the red, thus insulated, is of too feeble an illuminating power to effect the sight in the immediate vicinity of the other more brilliant rays, and only becomes visible when they are extinguished, or greatly enfeebled. To an eye defended by such a glass, vision, through a prism with the largest refracting angle, is as sharp, and the outlines of minute objects as free from nebulosity and indistinctness, as if their rays had suffered no refraction. These characters,—the absolute homogeneity of the rays,—their situation precisely at the least refracted limit of the spectrum, and the facility with which they may be insulated, render them of peculiar importance as standards of comparison in optical experiments."

In this simple and unpretending manner does Mr Herschel announce, what we must consider one of the greatest accessions to the catalogues of optical facts, which has been made since Newton first pointed out the unequal refrangibility of the primary rays. To their number Mr Herschel has added another, whose existence had not previously been suspected: in the analysis of light he has detected a new ingredient, and has thus found a new and exact means of measuring the dispersive powers of different media. To this purpose he has, at the conclusion of his paper, applied the insulation of these extreme red rays, and of the extreme violet: the deviation thus obtained, being of course greater for every sort of glass than any obtained by former methods, and the measurement extremely exact, from the circumstance of the rays being precisely defined and truly homogeneous. The method of operating is, we believe, new, and very simple.

The utility of the extreme red rays for this purpose is unquestionably very great; but the fact will be interesting to philosophers in a variety of other points of view. We have already made some remarks on the decrease of illuminating intensity in the different spaces of the spectrum, from the centre to the extremities: this is closely connected with the existence of invisible rays. It has been ascertained that the eye is somewhat deficient in its power of converging red light: from this cause alone, if the red rays were presented to it in an insulated state, the outer part of the red would be indistinct, and it would be very probable that certain extreme rays might exist which would be altogether invisible; but whe the rays are presented in juxta-position, the influence of the central rays which converge at a shorter distance will tend to increase the deficiency in the perception of the extreme red; and this would be the case on the supposition that all the rays possessed an intrinsic equal illuminating power and were all of equal density: but if in this respect they differ, (as we have seen they do,) the diminution will be still more considerable. It would thus be evident, that at whatever distance from the central point the real termination of the spectrum were situated, the apparent illuminating powers must decrease by a much more rapid law, than the absolute and intrinsic intensities would do: so that the apparent limit of the spectrum, would be at a much shorter distance from the point of maximum illumination.*

The discovery of the new red rays has, as might be expected, excited great interest; they have been recently examined by Mr Powell, who has measured their deviation, and observed them also, in the moon's light. In forming the spectrum as in Dr Wollaston's experiment above described, their appearance is remarkably distinct; in the spectrum of the blue part of a flame they do not exist, although there is much of the more refrangible red.

On the peculiar importance of these rays, in respect to another branch of physical inquiry, we will not here enlarge, as we believe we shall shortly have a more appropriate occasion of noticing them.

Meanwhile we have to trust to the indulgence of our readers for the length to which our present subject has led us; but we can assure them we have used our utmost endeavours to compress into the shortest compass, all that appeared to us most likely to be generally interesting in the recent history of optical researches.

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* For some able illustrations bearing on this point, we refer our readers to a paper by Dr Brewster, "On the Adjustment of the Eye," Edinburgh Journal of Science, No. 1, p. 77.

Art. XXXIII.—*Remarks on the Defoliation of Trees.** By the Rev. John Fleming, D. D. F. R. S. E., &c. Communicated by the Author.—[*Brewster's Jour.*]

The arrangements which prevail in the vegetable kingdom regarding the "Duration of Leaves," do not appear to have been studied with so much care as many other subjects connected with the economy of plants. Aristotle knew that some leaves were deciduous, others not. Cæsalpinus that many trees lose their leaves in autumn when their fruits are perfected, and their buds hardened, while such as retain the fruit long, keep also their leaves, even till a new crop is produced, and longer, as in the fir, the arbutus, and the bay. Linnæus distributes leaves in reference to their duration, into *decidua*, *caduca*, *persistentia*, *perennia*, *sempervirentia.* Sir James Edward Smith simplifies this arrangement of Linnæus, and considers leaves as to their age as of two kinds. *Sempervirens*, evergreen, permanent through one, two, or more winters, so that the branches are never stripped, as the ivy, the fir, the cherry laurel, and the bay. *Deciduum*, deciduous, falling off at the approach of winter, as in most European trees and shrubs.

It is much to be regretted that the learned botanist last mentioned, should have passed over this division of the character of plants without improving the classification of his master, or expressing himself more consistently with the phenomena of nature. A little attention to the subject will convince us, that winter is not the proximate cause of the falling of the leaf in many trees; that many leaves fall off long before the approach of winter, and that others which have withstood its attacks, perish with the warmth of spring. The want of attention to these circumstances, so conspicuous among our systematical writers, and which I have witnessed among well informed practical botanists, may serve as an apology for the following remarks. Perhaps they possess no novelty to those who are acquainted with the labours of the more recent physiologists. To others, however, they may appear to have a little interest.

Trees, in reference to the duration of their leaves, appear capable of division into three classes. In the first class may be included those in which the leaves cease to exercise their functions whenever the bud has been perfected. In the second the leaves continue to exercise their functions until new ones are produced in the following season. In the third class, the leaves continue to exercise their functions for several years.

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* Read before the Royal Society of Edinburgh, December 25, 1825.

In trees of the first class the leaf may with propriety be termed "*Folium deciduum.*" Its principal function appears to be connected with the ripening of the bud, and, when this object is accomplished, the leaf changes colour and dies. The falling of such leaves takes place, as, indeed, in all other cases, in the order in which they were evolved. Hence by midsummer we witness, in willows, for example, a considerable portion of the lower part of the shoot naked, its leaves having fallen, while towards the extremity its foliage is fresh and increasing in quantity.

In those trees in which two evolutions of buds take place in the course of the season, as in the beech, the leaves formed on the spring shoot change their colour and die sooner than those evolved at a later period on the summer shoot.

In those trees which seem to lose all their leaves suddenly, as the ash, the same order of defoliation actually prevails. The first evolved leaves of spring have perished, by degrees, in the course of the summer. Those with which the tree is clothed at the end of the season, are connected with the terminal buds, which by becoming perfect about the same time, permit the leaves connected with them to fall off in rapid succession.

The leaves, in some cases, after they have ceased to exercise their functions, continue attached until the following spring, as the beech in hedges. When this plant, however, is not under the influence of the shears, the leaves fall off in the usual manner. The cause of this want of resemblance between individuals of the same species, may be found in an examination of the different circumstances in which they are placed. When the beech grows exposed to free air and sunshine, the buds attain their true size, the leaves execute regularly all their functions, and drop off when they cease to act their part in the economy of the plant. But when the beech is trained in a hedge, it is too much shaded, the buds seldom attain their true size, and the leaf is frequently destroyed by cold, previous to the end which it is destined to serve having been accomplished.

In some cases, leaves, which naturally would fall off in autumn, continue, when the plant is subjected to the shears, to outlive the winter,—as the privet. The leaves, under such circumstances, may be said, in the language of farmers, to be "kept back," and to be capable of resisting the cold, while useful to the economy of the plant. Many analogous examples occur among herbaceous plants.

In the trees with deciduous leaves, it is probable, that, during the period of their nakedness, the bark may be viewed as the aerating organ, destined to effect the escape of the superfluous carbon from the system. During this period they may be viewed as resembling "*plantæ aphyllæ.*" The bark, indeed, of the young shoots of several trees and shrubs, as the common broom, is so very like in colour and texture, to the leaves, as to render it, in all probability fit to supply their place for a time.

In trees of the second class, the leaf may be termed "*folium annuum.*" It exercises its functions until a new set of leaves be produced, and is then cast off in the ordinary order of seniority. Many of our most ornamental evergreens are of this description, as the bay, laurel, ivy, and holly. These are termed evergreens, because the plant is never left naked, and the leaves of this year exercise their functions, and preserve their colour, until the shoots of the following season have acquired their clothing. The leaves of the plants of this class, appear, therefore, to exercise a greater influence, in the economy of vegetation, than in those connected with the first. Here the plant requires the aid of leaves at all times, no other organs, in ordinary cases, appearing to be capable of exercising their functions, or acting as a substitute. Do these annual leaves exercise a greater variety of functions than the deciduous leaves? Or does the bark of a tree, with annual leaves, exercise fewer functions, than the same organ in trees with deciduous leaves.

When growing too much in the shade, or when subject to the influence of the shears, annual leaves may have the period of their life prolonged, so as to exercise their functions after the new shoots have evolved their leaves.

In trees of the third class, the leaf may be termed "*folium perenne.*" Its duration is not influenced, directly, by the perfection of the bud, nor the new supply of leaves during the following season. The leaves of two or more seasons, exercise, in trees of this class, their functions at the same time, and appear to be requisite for the prosperity of the stem. Our ordinary evergreen firs furnish very obvious examples of this persistent leafing. In trees of this description, the leaves seem to exercise functions even of a higher order, and continue to exercise these longer than in those of the two preceding groups. The tree here requires the leaves, not for a few months, or until new leaves be produced, but *leaves of different ages*,—two, three, or more years old.

In the trees of this class there is less regularity in the falling of the leaf, as to season, than in the two preceding ones. Few of the old leaves drop off, when the tree produces the shoots, and new leaves in spring. A greater number seem to perish about midsummer, and again on the approach of winter.

The succession in the fall of the leaves is, as in the other classes, in the order of their seniority on the same stem and branch. But sometimes only a portion (the first formed ones) of the leaves of one season drop off, the remaining portion continuing to exercise their functions during a longer period. On the same tree may even be observed leaves of three, four, five, six, or even seven years of age; and while a part of those three years old may be changing colour and dropping off, those seven years old may remain green and fresh. Those which are placed in the shade live longest, yet even in this respect I have witnessed many anomalies.

These remarks apply to the duration of the leaves of the trees of this country. The influence of climate on the duration of leaves has often been stated as considerable, and capable of converting deciduous leaved trees into evergreens, or the reverse. In cold climates, vegetation is interrupted by winter. In warm climates, plants experience not such interruptions. It may therefore be asked, Will a warm climate alter the character of the leaves of certain trees, so far as to change a deciduous into an annual or perennial leaf? Or is there a source of deception arising from the *continued vegetation* exhibiting trees as evergreens, though in fact their leaves be deciduous? An affirmative answer to the latter question, will probably be found an expression of the truth.

Art. XXXIV.—*Essay on Calico-Printing.* [*Parkes' Chemical Essays.*]

(Concluded from page 251.)

Another kind of process remains to be noticed, called chemical-discharge work. Here the cloth is first died of some uniform colour, by means of a mixture of iron-liquor, and some one or more of the common vegetable dyeing substances; and calicoes thus prepared are said to be dyed of self-colours. They are then washed and dried; and when properly pressed or calendered, they are fit for receiving any pattern whatever, according to the taste or design of the artist.

This is generally effected by means of the mineral acids, which are previously prepared for the purpose, by dissolving in them a portion of one or more of the metals, according to the nature of the dye which is intended to be discharged, or of the colour to be produced. In doing this, care is taken that the discharging liquor be made so as to be capable of dissolving the iron which is contained in the dye, and which is always used in such quantity as to cover, or at least to disguise in a great measure, the other colour or colours which had been employed with it, and at the same time to act as a mordant in beautifying and fixing those colours.

Thus a piece treated with a decoction of Brazilwood, and dyed black by being padded* with iron-liquor, if, when dried, it be printed with a peculiar solution of tin, the ferruginous portion of the dye will be dissolved, and the printed part will instantly be converted from a deep black to a brilliant crimson.

In the same way an olive-coloured calico, dyed in a solution of iron and a decoction of weld, will as quickly be changed to a pale bright yellow; and the various drabs and slates of every shade which have iron in their composition, will undergo as sudden a change by the same treatment; though the colour of the figures produced on them will depend on the materials with which the cloths were originally dyed. Even the deepest gold colours, or strongest buffs, if produced by iron only, may, by a peculiar preparation of tin, be discharged; and such parts of the cloth as have been treated with this metallic solution will be restored to their pristine whiteness.

By similar management, calicoes dyed of a light blue in the indigo-vat, then run through sumach and coperas, and finished in a bath of quercitron bark and alum, may have figures of a bright green imparted to them. Here the green is originally formed by means of the indigo-vat and the bark, though it is enveloped by the iron of the copperas, which overcomes the other colours, till the solution of tin is applied, which removes the iron from those particular parts, and gives a brilliancy to the remaining colours which they would not otherwise have possessed; the tin being a powerful mordant for the bark, by which the yellow of the green is produced.

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* By the term *padding* is understood the operation of passing the pieces from a roller through a trough containing a solution of iron, or any other mordant. *Blotching* is another term used in calico-printing, and is synonymous with padding.

Again, a good self-colour may be given to calicoes, merely by dyeing them in sumach and copperas, and then running them through an alkaline solution of annotto; and here the figures produced by the application of a colourless solution of tin will be of a bright orange. But it is needless to enumerate more instances, as the workman accustomed to a dyehouse will have little difficulty in varying these in a thousand ways, when he becomes acquainted with the nature of the solution of tin which he employs.

The whole of this, however, refers to that branch of discharge-work only, where all the purposes are attained by dissolving the iron which makes a part of the colour that is intended to be discharged; whereas, the finer and more expensive work is done in a different way, and by a process which it will be necessary for me presently to describe.

It is, however, proper to remark, that there is an objection to the particular kind of chemical discharge-work of which I have been speaking, namely, that it is not perfectly fast; that is, the goods produced in this way will not bear such frequent washing, as those which are done by the bath of madder or bark.

It is certainly an object of great national importance to give a permanency and beauty to the calico-printing of the country;* and a great deal of very excellent printing is now done in various parts of the kingdom, especially for the best chintz-work and other furniture patterns. But, in what is called fast-work, there is a great variety of qualities, and some of it little deserves the name of permanent.

The mention of permanent colours reminds me of a very valuable GREEN which was invented some years ago by a Mr Islet of London, and which deserves to be noticed in this

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* To this end great improvements have of late years been made in the method of grinding madder-roots, by separating the inferior parts and dividing the whole into two or three different qualities. Thus the printer is enabled to apply the finest, which in this way is made equal to Dutch-crop-madder, to his best work, and the other can be laid by for inferior purposes.

Essay. This colour, which was secured to him by His Majesty's Letters patent, is produced by printing ground indigo mixed with a peculiar kind of solution of tin, and in then fastening the indigo within the fibres of the calico by means of that process, which is well known to printers by the technical designation of *china-blue dipping*.* After this the goods are dyed in a copper of bark or weld, which converts the blue to a green, and the whites are afterwards cleansed by croft-bleaching, &c.

Having myself formed a very high opinion of this invention, I had several interviews with Mr Islet, soon after he obtained the patent, and from him was fully informed of the whole process.—This I have since repeated for the purpose of verifying the detail in all its branches; and I am satisfied that it is one of the most beautiful and PERMANENT colours that has ever been fixed upon cotton.

There is another mode of producing very beautiful blues which has been much practised lately, and therefore deserves notice. This consists in printing some solution of iron, and then passing the goods through a very dilute and neutral solution of prussiate of potash. The Prussian blue which is thus formed upon the cloth may be rendered tolerably permanent by a variety of expedients, and this by means of any of the yellow dyes may afterwards be formed to any shade of green or of olive.

Begging to be excused for the irregularity of these digressions, I must not forget to revert to that other kind of discharge-work which I have engaged to describe, and which I will now attempt as concisely as is consistent with perspicuity and correctness.

Here, the agent which is employed is the citric acid, and this is used in various states of concentration according to the purpose to which it is to be applied, and the strength of the ground intended to be discharged. It is chiefly employed for the production of white figures upon self-coloured grounds produced by madder and sundry other dyes. For this intention the acid, in whatever state of concentration it may be, is mixed with either gum or with flour-paste† to a proper consistency for the block, the plate, or the cylinder, and from thence it is transferred to the piece; and wherever it attaches, the mordant, whether iron or alumine, is discharged, and a delicate white arises in its stead.*

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* China-blue is produced thus :—Indigo ground fine, and then thickened, is printed upon the cloth, and afterwards it is dissolved, and chemically united to the fabric, by alternate immersion in a solution of sulphate of iron and in lime water.

† When citric acid is used for resist-work, it is always mixed with gum senegal and pipe-clay. The clay gives it a greater body, and likewise acts mechanically as a resister.

The acid here referred to is produced from the juice of limes or lemons, and formerly it was not employed by the calico-printer until it had been reduced to the utmost point of concentration, and appeared in a crystalline form. Even then, it was not thought sufficiently pure, but was dissolved again, and redissolved and recrystallized, until it became as white and pellucid as any other pure salt in a crystallized state, and was then generally sold for 36*s.* the pound, at which high price it could only be employed on the best styles of work. Now, however, it is oftener used in the brown, or first state of crystallization; and some of the larger printers purchase lime juice, and concentrate it themselves; and in many cases they use it largely both for discharge- and resist-work, without crystallizing it. More on this subject may be seen in the Essay on Citric Acid, which was written with the hope of rendering an essential service to a large body of manufacturers in various parts of these kingdoms.

This mention of discharge-work by citric acid,† reminds me of another species of discharge, which is employed by the printers of Bandana handkerchiefs, and which I am under the promise of noticing before I conclude this Essay.

The agent which these printers employ is the nitrous, and sometimes the nitro-muriatic acid. It is used for the purpose of putting yellow figures upon blue silk handkerchiefs. The following is the process which is principally adopted.

Aqua-fortis, or nitro-muriatic acid, of such a strength as is suitable for the kind of blue which is intended to be discharged, is mixed either with gum tragacanth, or with flour-paste, to a proper consistence, and in this form it is printed on the silk, by means of a common block, on which the intended pattern is cut. The consequence of this is, that wherever the acid attaches, there the original colour is discharged, and a yellow dye is produced in its place. The pieces are then steamed, by passing them over a vessel containing boiling water, which gives brilliancy to the colour and finishes the operation.

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* It should be understood, that the discharge is printed upon the mordants before the goods are dyed. In using citric acid for this purpose, a portion of one of the mineral acids is sometimes mixed with it.

† Mr Thomson, a scientific printer, who has an establishment near Clithero, which is carried on under the firm of Thomson and Chippendale, has taken out a patent for discharging the Turkey-red dye by means of the citric and oxy-muriatic acids; and the work executed in this way has a very pleasing effect.

If a stronger dye than the usual yellow, or even a deep orange be desired, all that is necessary is to immerse the goods, for a moment, in lime water, or in a solution of lime and potash; and by varying the proportions of these ingredients a great variety of shades may be produced.

It must be noticed that this method is applicable only to the discharge of the colour from *silk* handkerchiefs; the process for producing the spots on *cotton* Bandanas being entirely different. This manufacture, which is of considerably more importance than the former, was established some years ago in Scotland, for the purpose of producing imitations of the Bandana handkerchiefs which are imported from India. It is conducted in the following manner.

Pieces of fine calico are dyed by the Turkey-red process, of a colour as nearly approaching to scarlet as can be produced on cotton by that means; and, upon this plain dyed calico, white spots are produced by a method to be desrcibed hereafter. The pieces of calico having been bleached to a perfect white before they are dyed Turkey-red, the present object is, not only to discharge the dye on certain parts of the cloth, but by the same agent to restore those parts to their original whiteness.

These discharged parts are usually in the form of spots, sometimes round and sometimes square or triangular, and these marks or spots are produced by the action of a chemical liquid, while the pieces of calico are under the action of a powerful press. The machinery for this purpose is made in an expensive manner, as it is required to be of very great strength, and it is put together with the utmost exactness.

To this press two horizontal plates are adapted, for the purpose of receiving the calico which is to be placed between them. The pattern intended to be given to the handkerchiefs is first marked upon these plates; and in the bottom plate holes are cut of the exact form and size of the intended spots; whereas, in the upper plate every spot is marked out by a small copper tube which goes through the plate and is firmly cemented within it.

When the plates are about to be fixed, there is a contrivance for ascertaining that the orifices of the tubes in the upper plate are exactly perpendicular to the respective holes in the under plate; which is a circumstance of the utmost importance, as the discharging liquor which is poured into the tubes is designed to pass through the folds of calico into the corresponding holes in the lower plate.

The apparatus being thus prepared, the handkerchief pieces are neatly folded into squares of the size of a single handkerchief and of the thickness of about ten or a dozen folds. One of these pieces, consisting of ten or twelve handkerchiefs, is then laid smoothly upon the lower plate, and is so disposed as to lie square with the plate. By means of a lever, the cover of the press, to which the upper plate is attached, is then lowered, and by continuing the action of the lever, these plates, with the folds of calico between them, are screwed firmly together.

The consequence of this is, that the ends of the tubes fixed in the upper plate, are brought to press upon the surface of the cloth; and as these are made to press upon it with great force, and as each tube has a corresponding hole immediately beneath it in the under plate, it is evident that the fluid which is poured into these tubes will be directed into the holes below, and cannot pass in any other direction.

When things are thus arranged, and the press is firmly screwed down, a very dilute solution of chloride of lime, containing a small portion of sulphuric acid, is poured upon the upper plate of the press; and as this plate has an iron rim round its upper surface, the fluid which is poured upon it is determined into the tubes, and as it percolates through the cloth it discharges the colour on those particular parts, and passes off through the holes of the lower plate into a trough placed underneath to receive it.

It is observable that the chemical liquid is prepared sufficiently strong to discharge the colour in the course of about ten or twelve minutes, and yet not so powerful as to endanger the fabric of the cloth; and that the great pressure of the screw prevents the fluid from spreading beyond the precise limits marked out by the tubes and the corresponding holes underneath.

As soon as the discharging liquor is found to have entirely passed through, the press is slackened, and the cloth is removed to make room for another piece, which is immediately substituted; and in this way the process is continued throughout the day without interruption. One press, requiring the attention of two men only, will print fifty pieces, containing one dozen handkerchiefs each, in the course of the day.

When the pieces are first taken from the press, the spots which have been produced by the discharging liquid are not perfectly white; it is therefore necessary to have recourse to a subsequent process similar to the one usually employed for clearing the whites on common printed calicoes; and this not only removes every stain, but also heightens the brightness of the Turkey-red dye. I am aware that at one time there was a general complaint of such handkerchiefs, after frequent washings, becoming tender in those parts which had been acted upon by the chemical liquor: this I think must be owing either to the employment of a liquor too much concentrated, or to the remains of the acid not having been perfectly removed before the goods were dried and pressed for sale; as I am persuaded that with due caution the process may be safely conducted.

When speaking of yellows, it was my intention to have mentioned, that there is a mode of producing yellows on calico which is not very frequently practised, and yet has a very good effect. The process is as follows:

A strong decoction of quercitron bark, thickened with gum tragacanth, is to be mixed with a portion of very pure muriate of tin, and this, when printed with the usual management, will produce a colour of great brightness and durability. I mention this the rather, because very many pleasing effects may be obtained by this method which cannot be produced in the usual way, by means of the acetate of alumina, and any of the yellow dyes that may be employed with it.

There is one very important advantage which this mode possesses, viz. that should it be necessary to pad a piece in diluted acetate of alumina to obtain a pale lemon ground, the yellow figures, previously imparted by the above process, will not give out any part of their colour to the second mordant; whereas, whenever a strong yellow has been produced in the common way, the pattern is very apt to spread and become irregular, and oftentimes to stain the white ground, when the piece comes a second time into the acetate of alumina.

The most expensive kind of calico-printing called CHINTZ-WORK still remains to be described. The term chintz-work is descriptive of that kind of calico-printing which is employed for window-curtains and other furniture, and it differs more in the richness and variety of the colours, than in any other circumstance.

In relating the processes by which these beautiful prints are produced, I shall suppose the calico to be already properly bleached and smoothed ready to receive the impressions of the block. The first thing then to attend to, is to apply the mordant for the colour which is intended to be imparted in the first instance. Thus, if a black be designed, a mordant of acetate of iron, commonly called iron liquor, is thickened with gum, and printed upon the cloth in any pattern that may have been selected for the purpose. If this same mordant be diluted with water, it will form a proper mordant for a purple; and the same, still further diluted, will, when it comes into the dyeing-copper, form a lilac. In this way, all the varieties of shades from a pale lilac to a strong purple, and from a purple to a black, may be produced by acetate of iron diluted with various portions of water, and then dyed with madder.

In like manner, a colourless solution of acetate of alumina thickened with gum or flour-paste, forms a mordant for dark red; if diluted with water, it makes a common red; and by diluting it further and further, every shade of pink may be produced. Again, by the admixture of acetate of iron and acetate of alumina, a mordant for chocolate colours, maroons, &c., is formed, either approaching to the purple or the red, according to the admixture; that is, according to the proportion of either of these original mordants which may predominate in the mixture.

When these several mordants have been printed upon the calico, they are allowed to dry for two days or more in a stove, or drying-house; they then go through the operation of dunging, which consists in rinsing them in warm water in which a little cow-dung is diffused, as has been already described. When the pieces are sufficiently dunged, which is not the case till all the superabundant mordants are removed, they are well washed in clean water, and then boiled in a decoction of madder, until the madder-bath is exhausted. In consequence of different mordants having been applied to the cloth, this one boiling in the madder-liquor will at once produce all the colours above mentioned. When the pieces are thus dyed, they are to be rinsed in cold water, and laid upon the grass to bleach. By this exposure to the air for a few days, the whole of the ground to which none of the mordants had been applied, will become perfectly white.

The processes which have now been detailed, will produce what is called *common* chintz-work; but if it be desired to make the goods still richer, by the addition of yellows, bright olives, drabs, &c., the cloth must undergo another series of operations, which may be thus described:

Upon those parts of the calico which still remain white, any of the above mordants may be printed, according to the effect designed to be produced; after which, all the preceding managements are to be repeated, except that instead of boiling in a decoction of madder, they are to be immersed for about half an hour, more or less, in a warm decoction of quercitron bark, (the *Quircus nigra* of Linnæus,) a most important dye-wood, introduced by Dr Bancroft, and which is found to give out a much brighter colour to *tepid* water than it does when treated with *boiling* water, or with water nearly approaching to that temperature.

The effect produced upon these prints by an immersion in a lukewarm decoction of this American bark, will be quite different from that produced by the madder; upon those parts of the cloth where the mordants have been printed which before produced a black, a dark olive will only be apparent; and instead of pompadours will be drabs, and instead of reds we shall have yellows, which will vary in intensity according to the strength of the aluminous mordant.

Again: a further variety may be given to these prints, if the yellow mordant, or acetate of alumina, be applied to any of the colours which have already been dyed with madder; but this must be done before the pieces are immersed in the decoction of bark. This application will convert the reds and pinks into different shades of oranges, and the lilacs into cinnamon colours. By means of these different processes, an endless variety may be given to the goods, and a calico-printer of taste will never be at a loss how to produce a pleasing effect, whatever may be the patterns which he has to imprint upon the cloth. The second immersion in the dying vessel will, however, give a yellow tinge to the remainder of the whites, but a short exposure on the grass will obliterate it.

When chintz-furniture-prints are designed to have as much variety of colouring as possible, a part of the remaining white is often coloured blue or green, or of any shade between those colours, by a still different process. This is done with what is called pencil-blue, which is a preparation that has already been described. The blue is given by putting in the prepared indigo with a pencil; and the green is produced by pencilling some of the same colour over certain parts of the pattern which has already been dyed yellow.

When these colours have been imparted, the printing is said to be finished, and the pieces are hung up to dry for at least twenty-four hours; after which they are rinsed thoroughly in cold water; and when they have been dried with care, they are properly calendered and put up for sale.

Nothing now remains but to notice an improvement, which has been made of late years by the introduction of CYLINDER-PRINTING, which has the advantage of superior accuracy and neatness, as well as of great expedition.*

The machines which effect this are rather complicated, and very expensive; but some kind of work cannot be produced at so cheap a rate by any other means. They are so contrived that the cylinders on which the patterns are engraved, furnish themselves with the prepared colour during their revolutions; their surfaces are kept clean by a steel knife, or *doctor*, as it is called, passing over them the moment they have charged themselves with the thickened colour; and they have such a pressure given to them, either by means of screws or levers, which can be tightened or slackened at pleasure, that the whole cylinder is made to deposit its colouring matter with the greatest certainty and exactness on the cloth, while this rolls over it in succession, from one end of the piece to the other.

These cylinders, which are made of copper, are from eighteen to forty-two inches in length, according to the width of the calico to be printed, and from three and a half to five inches in diameter; and these massy rollers have the patterns enchased upon their surfaces, in the same way as a pattern is cut upon a flat plate of copper that is intended to be employed in copper-plate printing.†

Many of these machines are contrived so as to carry two of these cylinders, each of which has a trough of colour attached to it, by which means two different colours may be printed on the same calico, at one and the same time; and Mr Adam Parkinson of Manchester has invented a machine capable of printing at one time, by means of one cylinder, and two surface rollers, or by two of the former and one of the latter, three distinct colours.

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* A variety of observations on the printing of calicoes by means of the cylinder, which may be read with advantage in connexion with this essay, will be found in Nicholson's 4to Journal, vol. i. page 18.

† As these cylinders are made with plates of copper hammered into a circular form and joined by brazing, great loss has sometimes been sustained by the engraving giving way upon the brazed joint. To obviate this, a patent has been obtained for boring the copper cylinder from the solid metal in the modern way of boring cannon.

These machines have not only the excellence of printing more correctly than can possibly be done by means of the block, but the saving of time and labour which they afford is great indeed. A piece of calico, which would take a man and a boy three hours to print with one colour, or six hours to finish with two colours, may by this means be printed in three minutes, or three minutes and a half, and the work will be much more completely done than could even have been imagined before the introduction of this invention.

Besides these cylinders, there are others, which are called SURFACE-MACHINES, which contain cylinders, not of copper but of wood, and which have the pattern formed upon their surfaces in relief, exactly similar to the blocks described in page 239. These are employed in particular styles, especially on light grounds,* and for certain kinds of resist and discharge-work.

It must be obvious to every one who is acquainted with the subject, what an astonishing facility these machines must have afforded to the production of printed calicoes; and also what an advantage they give to the British printer in foreign markets.

ART. XXXV.—*On the Barometer.* By J. F. DANIELL, F. R. S. [*Jour. Roy. Inst.*]

[Concluded from page 260.]

I NOW feel that I ought not to allow this opportunity to pass without making a few observations upon the new meteorological register lately published in the *Philosophical Transactions.* I am aware that, in so doing, I shall run the risk of being again designated as *hostile* to the council of the Royal Society; but I am encouraged when I consider that it is only by inferior minds that the correction of errors, and the sugges-

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† In light work, the white grounds are apt to be soiled by the cylinders: hence surface-machines were contrived, and these are not liable to the same objection. Cylinder-machines are more commonly employed in those styles which are full of colour and leave but little white.

tion of improvements, in the pursuits of science, can ever be considered as acts of hostility.

Great expectations, it is well known, were raised when it was announced that a committee had been appointed, consisting of the leading men of science, to take into consideration the state of the meteorological instruments and register of the Royal Society. I do not hesitate to affirm, that the failure of an attempt, about this time, to establish a society to promote the science of meteorology exclusively, was wholly owing to these expectations. Many persons, who would otherwise have concurred most heartily in the plan, waited to see the result of labours which they doubted not would effect the object in view; and feared that the new society might bear too much the features of opposition :—as if any thing, which had in view the promotion of science, could be considered as opposed to the Royal Society! The unusually-protracted time of the publication of the register contributed to keep alive these expectations. At length, in the beginning of the year 1825, appeared the long-looked-for journal of 1823,—but no report from the committee! not one word of preface! not a syllable about the instruments! A most disheartening similarity in the appearance of the arrangement is the first thing to strike the eye; and it is only by a close examination that it can be inferred that changes, and those very important changes, have been made.

The register is divided as before into ten columns, and the improvements commence with the first, which contains the dates of the month. To these are now prefixed the signs of the planets, to denote the days of the week! The alteration, at all events, is harmless; which, I fear, is more than can be predicated of all the others. From the second column we may collect that the times of observation have been changed to 9 A. M. and 3 P. M., except not a few instances, in which it is to be presumed that these hours did not suit the observer's convenience. Why the change was made is left to conjecture. The title of the third column announces, for the first time, that the barometer is "corrected." But how corrected? Is it the instrument itself which is corrected for any faults in its original construction? or its indications, which are corrected for adventitious circumstances? Is it corrected for capillarity? for temperature? or for any variation in the level of the mercury in its cistern? If it be a new barometer, is it of the siphon or cistern construction. If of the latter, what are the relative capacities of its tube and cistern? what the diameter of its tube? Is there any and what difference between its indications, and those of the old instrument? A comparison of the utmost moment. The only criterion by which we may conjecture that a new instrument has been substituted for the old one is, that the height is now registered to thousandths instead of hundredths of an inch. But will it be believed that the council of the Royal Society would suffer such gross negligence to appear not only under the sanction of their "order," but after all the pomp and circumstance of a committee specially appointed to superintend the necessary arrangements? The fourth and fifth columns contain the indications of the thermometers. The latter is headed "Thermometer Without." Whence, I presume, it is fair to infer that the other is, as before, "Thermometer Within." And yet this is entitled, in some places, "Six's Thermometer," and in others, "Register Thermometer." And we are told in the first note, "Six's thermometer deranged, and a horizontal register thermometer substituted for it." But then what does it register? There is but one observation in the day recorded; it cannot be the *maximum*, because in many cases the "Thermometer Without" is higher. It cannot surely be the *minimum*, for who would take that from an interior thermometer? However, before three months have elapsed, note the second informs us, "Register Thermometer deranged." Then comes an *hiatus valde deflendus* of a month, and we return once more to Six's thermometer and two observations per day—but how repaired, and where placed, we are not informed.

The sixth column is entitled "Daniell's Hygrometer," and contains, I presume, though nobody but myself is bound to conjecture this, the dew-point.

The seventh column records the "degrees of moisture," but whether calculated from the same instrument, or from any other hygrometer, is not stated. If from the former, the situation of the thermometer by which the calculation is made should have been most particularly determined.

The eighth column contains the register of the rain; and from the greater frequency with which the amount has been lately entered, we may conclude that the soot from the old chimney-cowl, under which the gauge is situated, is more frequently removed from the pipe than it used to be.

The ninth and tenth columns, recording the direction and force of the wind, bear every mark of their former accuracy; and the only remarkable fact is the very rare occurrence of any variation of the strength from the standard 1.

The eleventh and last column rings most edifying changes upon "rain," "cloudy," "fine."

The results of all this labour are summed up at the end of the journal in one short table, containing the means and extremes of the months, and the mean results of the year. From what data, or from what part of the register, the means of temperature are collected, it is very difficult to conjecture. From the note at the foot of the last page we learn that the barometer is now 100 feet, instead of 81, above the level of low water spring-tides at Somerset-House; and that the rain-gauge is still 114 feet above the same level; but by some chance or other, six inches nearer the ground than before.

The importance which attaches to such minutiæ as these, when undertaken by such a body as the Royal Society, cannot be better illustrated than by a circumstance which has lately been discovered, in determining the length of the second's pendulum; a measure upon which depend all the late parliamentary proceedings for regulating the weights and measures of the united kingdom.

The council, by whose orders the height of the barometer above the level of the tide was determined, little foresaw at the time that this simple operation could have any reference to proceedings of such importance; and yet hear what Captain Sabine says.

"The height of the pendulums in Mr Browne's house, in London, being here described as 92.5 feet above the level of the sea, whilst in Capt. Kater's memoir in the *Philosophical Transactions*, it is stated to be 83 feet only; it is necessary to explain that Capt. Kater's estimation of the height was founded, in part, on the understanding (*on the authority of the Royal Society*) that the elevation of their barometer at Somerset-House is 81 feet above *low-water mark;* but as the latter elevation has been since corrected by Mr Bevan, who has determined it, by levelling, to be 90.5 feet above the *mean level*, the height of the pendulums must now be considered as 92.5 feet, and is so esteemed by Capt. Kater."*

The same national work is also much affected by the want of such standard instruments as it is the appropriate province of the Royal Society to provide and preserve. Is it to be tolerated that results of such national importance should be made to depend for their verification upon a comparison with a thermometer, the property of a private individual? The uncertainty in the experiments arising from such a cause may, according to Captain Sabine, amount to "not less than $\frac{4}{10}$ths of a vibration per diem; being greater, as he had reason to believe, than the sum of the uncertainties due to all other causes whatever."*

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* Experiments for determining the figure of the Earth. By Edward Sabine, &c. p. 343.

Surely these considerations, urged from so many quarters, must at length excite the dormant energies of those to whom the honour of the Royal Society is committed. If it be more consistent with the dignity of that venerable body to give up the working departments of science, and to sit as judges only of the exertions of others, let them announce such intention openly, and there will then be many come forward in the field from which they retire. In most branches of science this is the course which has been already adopted; and yet they have, perhaps, enough to do as impartial dispensers of those honours for which there are so many competitors. But if they are still determined to persevere in causing observations to be made "by their order," in the only branch of natural science which now remains to them, let them at least provide that they be made with all the care and precision which the actual state of that science demands; for upon this the honour of the Society is at stake.

Art. XXXVI.—*Mr Children's Summary View of the Atomic Theory according to the Hypothesis adopted by M. Berzelius.* [*Ann. Phil.*]

(Concluded from page 271.)

The symbols of organic atoms are denoted, like the inorganic, by the respective initial letters of the Latin names of the substances, but are distinguished from the latter by a line drawn above the letter. Thus $\overline{A}$ = acetic acid, $\overline{C}$ = citric acid, $\overline{T}$ = tartaric acid, &c. So much for the symbols. That they are on the whole ingeniously contrived we do not mean to deny, but that they are necessary or useful we are much disposed to question. At all events they should be consistent with themselves, and the anomaly which we have pointed out between the sulphates and silicates, should, as it very easily might, be done away. If a cipher be required, the key should be constant, and apply equally to every part of it; but there are in our opinion many and serious objections to the adoption of symbols at all. In the first place it requires great care to write them correctly, for a single error may wholly pervert the meaning of a formula, and the consequence of such an error is the more serious, because it cannot, as in common language, be readily detected and corrected by the context. The errors of the press too are more likely to escape notice, and thus this species of danger, from inaccuracy or inattention, becomes doubled. Much habit is required both in writing and reading the symbols, as well as considerable application to become so familiar with them as instantly to comprehend their meaning, especially of the more complicated formulæ; and after all, what is the great benefit they are supposed to confer? A brief and easy method of stating the exact composition of all chemical compounds. For the brevity, it is more than counterbalanced by the risk of error; for the facility, it requires considerable study to learn to do that in one way which every body knows how to do in another without any study at all. But it expresses the exact composition of every substance in all its minutiæ—the number of atoms of bases and acids, of the electro-positive and electronegative elements—and they are too complicated to be expressed in common language without a tedious multiplication of words. They do indeed express the exact composition assigned to the various compounds by the hypothesis of Berzelius; but may not all those compounds be reduced to much simpler forms, and consequently the necessity for expressing them by this short-hand character be done away with? We shall try this question most fairly, by comparing the results of two or three analyses calculated on Berzelius's system, and on the more simple views adopted in this country; and for this purpose we may take some of those copied from Beudant in the preceding pages, and first that of a variety of emerald (see p. 263). The weight of an atom of

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* Experiments for determining the figure of the Earth. By Edward Sabine, &c. p. 192.

Silica . . . . = 16
Alumina . . . . = 17*
Glucina . . . . = 26

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[*] The weights of the atoms are from Philips's table, except that of alumina, which we take from Berzelius for the reasons given in the note, (p. 289.)

Proceeding on the principles already explained, we obtain the following quotients by dividing the quantity of each substance, as found by the analysis, by its proper atom.

Atoms.

$\frac{6864}{16} = 429 = 8$ of silica.

$\frac{1796}{17} = 105 = 2$ of alumina.

$\frac{1340}{26} = 51 = 1$ of glucina.

The sum of all the atoms = 188,* and by the rule of simple proportion,† we find the theoretical composition of the mineral to be per cent.

Silica	68·09
Alumina	18·09
Glucina	13·82
	100·00

Analysis of zoisite (p. 264.) The atom of lime = 28.

Atoms.

$\frac{4300}{16} = 268$ 3 of silica.

$\frac{3300}{17} = 194 = 2$ of alumina.

$\frac{2400}{28} = 85 = 1$ of lime.

The sum of the atoms therefore = 110, and the theoretical composition of zoisite is,

Silica	43·63
Alumina	30·90
Lime	25·45
	99·98

It is needless to multiply instances, as any analyses compared in the same way must obviously give the same results.

Thus we see that all the complicated statements, and still more complicated reasonings, on which they are founded, of which we have given examples in the preceding pages, may be just as well expressed with much greater simplicity, and that the simple statements equally furnish us with a test of the accuracy of our analyses. Our after reasonings as to the mode in which the elements are severally united in the actual mineral, however probable, can only be conjectural, and we are just as likely to form a correct estimate on the simplest as on the most elaborate system. What good purpose then do these complicated statements answer? Do they teach us more accurately the true constitution of mineral substances, or the mode in which their elements are combined? We cannot perceive how. They do indeed, as we have said of the formulæ, give a detailed view of their author's hypothesis, but do they therefore prove its accuracy? It seems to us to be reasoning in a circle. The formulæ are made for the hypothesis, and the hypothesis supports the formulæ; but what arguments can be deduced from both together to render it more probable that alumina and silica contain 3 atoms of oxygen, and lime, baryta, &c. 2, than that each of those substances is composed of 1 atom of base and 1 of oxygen? In point of fact, both views come to the same thing; for if we assume lime to contain 2 atoms of oxygen, the weight of the atom of the base (as stated above) must necessarily be doubled, so that in reality whether we represent sulphate of lime by the formula $\dot{C}\,\dddot{S}$, or $\ddot{Ca}\,\dddot{S}^2$, we equally express a triple ratio of the oxygen of the acid to that of the base, and so in all other cases; for $\dot{Al}\,\dddot{S} = \ddot{Al}\,\dddot{S}^3$. The adoption of the latter formula, therefore, is as if one should expect to approach nearer to the truth of a proportion by writing 999 : 666, instead of 3 : 2.

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* $16 \times 8 = 128$
$17 \times 2 = 34$
$26 = 26$
188

† 188 : 128 :: 100 : x, and $x = 68·09$, and so with the other atoms.

But perhaps it may be argued that the hypothesis presents a correct view of the analogies subsisting between all oxidated bases, and enables us to arrange them in separate orders according to certain characteristic properties by which the oxides of one order may be distinguished from those of another. The observations of M. Mitscherlich seem to demonstrate that such distinct orders actually exist, and as the subject is both curious and important, and because we would not willingly suppress any argument that may appear favourable to the hypothesis, we shall dwell a little upon it, although this paper has already exceeded the limits we had originally prescribed for it.

We have another motive also for doing so. We know that one of the first crystallographers of the present day* thinks favourably of M. Mitscherlich's theory, and our respect for his opinion would alone induce us to treat it with attention. It would give us great pleasure if that gentleman would take up the subject, and correct any errors that either ourselves or others may have fallen into concerning it.

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* M. Levy.

M. Mitscherlich observed that certain bases, saturated with the same acid to the same degree, affect the same crystalline forms, and that lime, magnesia, and the protoxides of iron and manganese compose in this manner one class of what he has called *isomorphous* bases; while alumina and the peroxides of iron and manganese form another. He showed also that isomorphous salts have the property of crystallizing together, concurring in an uniform manner in the formation of one and the same crystal. M. Mitscherlich supposed that the primary forms presented by isomorphous bases are really identical, and that this identity necessarily results from a similarity in their atomic constitution, that is, in the proportions of oxygen contained in the elements of the isomorphous crystals; and that wherever this atomic similarity exists, identity of crystalline form will always be the result. Thus he says the oxygen in the phosphorous and arsenious acids is to that in the phosphoric and arsenic acids as 3 : 5. In the biphosphate and binarseniate of potash, the oxygen of the base is to that of the acids as 1 : 5, and to that of the water of crystallization as 1 : 2.

Hence the only difference between these salts consists in the radicle of the acid of one of them being phosphorus, and that of the other arsenic; and all the salts, which differ only in this manner, are said to present identical crystalline forms.

Berzelius has made considerable use of Mitscherlich's hypothesis to bring together as one species all the varieties of garnet, as well as those of amphibole, mica, and several other minerals; and in vol. ix. New Series, p. 70, of the *Annals of Philosophy*, our readers will find an abstract from Wachtmeister's paper, in the Swedish Transactions, containing a description and analysis of 13 varieties of garnet, all of which, with only one exception, proved to be constituted of an atom of a silicate of a base containing 3 atoms of oxygen, as alumina and peroxide of iron, combined with an atom of a silicate of a base containing 2 atoms of oxygen, as lime, magnesia, protoxide of iron, and protoxide of manganese.

M. Beudant has the following remarks on the same subject. After observing that it is scarcely possible to obtain artificial salts in a state of purity by crystallization from a liquid holding several salts in solution, unless they differ very materially in point of solubility, in which case they crystallize in succession, one after the other, he says, "if on the contrary they are nearly equally soluble, they all mix together in greater or less proportion, and not one of them will be pure. These mixtures often happen indifferently with every species of salts, so that they appear to be the mere effect of chance, and in that case the extraneous portion is always in very small quantity. But mixtures occur under certain circumstances which it is very important to understand, and may then take place in all sorts of proportions, wherefore sometimes no particular ingredient sensibly predominates. In general it is observed that salts of the same order of composition unite most readily, especially when they have nearly similar crystalline forms. Thus all the species of alum have such a tendency to mix together, that it is extremely difficult to counteract it, and they cannot be completely separated when once united, even by repeated crystallizations. Mixtures of the same kind occur between nitrate of baryta and nitrate of lead; between the nitrates of potassa and soda; and the sulphates of iron, cobalt, nickel, &c.; also between the sulphates of zinc, soda, and magnesia, &c. &c. These mixtures not only occur when a solution contains merely the salts of the above-mentioned groups, but if a great number of salts be dissolved in the same liquid, they will form by preference, so that it may be said that salts belonging to the same order of composition, seek each other, as it were, to crystallize together, and mix in every proportion.

"In the case of the mixture of different salts of the same formula, it is observed that the crystalline forms are not sensibly affected, for such salts have, if not identical forms, at least forms of the same kind, and very nearly allied with respect to their angles; as was first observed by M. Mitscherlich. Hence we can imagine that at the moment of their becoming solid, a certain number of the molecules of one salt may be substituted for those of another without occasioning any irregularity in the crystallization. This identity of formulæ is not only observed between salts with the same acid, and having different bases of the same degree of oxidation, but also between salts of the same base, or bases of similar degrees of oxidation, that have different acids of the same order of composition. Whence it results that not only salts of different bases have analogous forms, more or less nearly allied, but also that salts having different acids are similarly circumstanced."*

"Mixtures of substances belonging to the same formula of composition are also extremely frequent in nature both in simple and multiple compounds; but as we cannot in this case, any more than in that of artificial salts, separate at will the immediate principles of these bodies, it is only by the consideration of their analyses that we can arrive at a knowledge of those mixtures. Now by this consideration, we find in the simple compounds, that such or such an oxide is replaced by such or such another belonging to the same order of composition. For instance, in stones accidentally coloured by a combined oxide, we find that the colouring principle is some oxide which replaces either that which serves as base, or that which plays the part of an acid. Thus in the silicates with base of lime, or the bioxide of calcium, the colouring matter is frequently the bioxide of iron, and its quantity is such, that its oxygen is precisely equal to that of the lime that is wanting. It follows that the sum of the oxygen of the lime, plus that of the bioxide of iron, is exactly equal to the quantity of oxygen, which the lime would contain in the pure colourless silicate. In silicates with base of alumina, or the trioxide of aluminium, the colouring matters are the trioxide of iron, the trioxide of manganese, &c. sometimes both; and their quantity is such that their oxygen is equal to that of the deficient alumina.

"In multiple compounds, one or other of the immediate principles is often replaced by one or more principles of the same formula, whose quantity varies indefinitely in different analyses, but is always such that its oxygen is equal to that of the principle replaced: hence if we take on the one hand the oxygen contained in the common acid, and on the other the sum of the quantities of oxygen contained in the bases, we obtain numbers which are precisely in the same ratio to each other that they would be if the compound were perfectly pure. We are even led to the knowledge of cases in which one of the immediate principles is replaced by another with a totally different acid, the base either remaining the same, or being itself different. Thus silicates of lime are replaced by aluminates of the same base, or by aluminates of bioxide of iron, &c."†

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* Traité Elementaire, &c. p. 244.
† Ibid. p. 248.

M. Beudant then goes on to illustrate the preceding observations by examples, and gives a sort of receipt for making garnets, or rather endeavours to show how one compounded of many elements may be divided into several others of more simple composition.

We shall quote, with some abridgment, his first example.

"There are garnets obviously of the following composition.

Silica	41	containing oxygen 20·60	or 2 atoms.
Alumina . . .	22	10·27	1
Lime	37	10·39	1
	100		

which indicates 2 atoms of silicate of alumina, plus 1 atom of silicate of lime; and a series of direct analyses presents a multitude of other results that can only be explained by calculating them on the atomic system. Thus the connexion between the following analysis and the preceding is by no means obvious.

Silica	37·00	containing oxygen 18·61
Alumina	13·50	6·30
Lime	29·00	8·14
Magnesia	6·50	2·51
Trioxide of iron	7·50	2·30
Trioxide of manganese	4·75	1·41
	98·25	

"If we collect the oxygen of the bases of the same order, namely, the alumina, trioxide of iron, and trioxide of manganese, on the one hand, and that of the lime and magnesia on the other, we find that the quantities of oxygen in the acid and bases are not far from the ratio of 2, 1 and 1, consequently the new garnet very nearly harmonises with the former.

"Further to illustrate our analysis, let us insulate each of the species of garnet contained in the mixture. If we employ the trioxide of iron, to make a melanite garnet (*en faisant un grenat mélanite*) of the formula $2\,\dddot{F}\,\dddot{Si} + \dot{Ca}^3\,\dddot{Si}^2$, we must take a quantity of

Trioxide of iron, whose oxygen =	2·3	corresponds to 7·50
Silica	4·6	9·14
Lime	2·3	8·19
		24·83

"There remains a portion of lime whose oxygen is 5·84, with which we may make a grossular garnet (*dont on peut faire un grenat grossulaire*) of the formula $2\,\dddot{A}\,\dddot{Si} + \dot{Ca}^3\,\dddot{Si}^2$, by taking a quantity of

Silica, whose oxygen =	11·68 corresponds to	23·22
Alumina	5·84	12·50
Lime	5·84	20·79
		56·51

"There then remains a quantity of silica whose oxygen is 2·33, and to use it up (*pour l'employer*), we may first make a garnet of alumina and magnesia (*on peut faire d'abord un grenat d'alumine et magnésie*) of the formula $2\,\dddot{A}\,\dddot{Si} + \dot{M}^3\,\dddot{Si}^2$, by taking a quantity of

Silica, whose oxygen =	0·92 corresponds to	1·83
Alumina	0·46	0·98
Magnesia	0·46	1·19
		4·00

"Lastley, we shall make of the remainder (*on fera du reste*) a garnet of manganese and magnesia, of the formula $2\,\dddot{Ma}\,\dddot{Si} + \dot{M}^3\,\dddot{Si}^2$, by taking the residual

Silica, whose oxygen =	1·41 corresponds to	2·80
Trioxide of magnanese	0·705	2·38
Magnesia	0·705	1·82
		7·00

"All these products subtracted, there only remains

Trioxide of manganese	2·38
Mangnesia	3·47
	5·85

which may be regarded as merely in the state of mixture;" and of which M. Beudant cannot *make* any thing further. He continues,

"Thus we see that the garnet in question contains

Melanite garnet	24.83
Calcareous garnet	56·51
Aluminous and magnesian garnet . .	4·00
Manganesian and magnesian garnet	7·00
Trioxide of manganese (mixed) . . .	2·38
Magnesia (mixed)	3·47
	98·19"

This is taking a peep into Nature's workshop with a vengeance, and it is really a pity that all the elements of the analysis could not be worked up; quite provoking that Nature should have employed nearly six per cent. of matter in *her* way of making a garnet, more than M. Beudant wanted for *his*, and still more so that all the elements should be in exact definite proportion in the first compound, and not in the last, so ingeniously dished up from the several ingredients of melanite, grossular, &c. &c. into this garnet olio! Other similar examples are given from the analysis of axinite and amphibole, but the reader will probably think the preceding quite sufficient.

M. Beudant concludes the chapter by observing, that the above method of discussing the analyses of minerals is the only way to form a clear idea of their composition—every other mode of looking at them, he says, "leads merely to vague ideas, or rather leads to nothing at all. The common plan of giving the weights of the insulated ingredients generally presents only a parcel of incoherences, and it is this bad method that has so long prevented the immediate application of chemical researches to mineralogy, by concealing all the advantages that may be derived from them." We strongly suspect we shall adhere to the *bad method*, notwithstanding.

With respect to the term *isomorphous*, M. Beudant very properly remarks, that it cannot be received in a rigorous sense, and that it frequently merely indicates a very strong analogy, the forms of substances, said to be isomorphous, differing only very slightly in the measurements of their corresponding angles.

The late M. Haüy was not a convert to the new views adopted by MM. Mitscherlich and Berzelius. After stating their ideas respecting pyroxene, he says,* "they were not led to these conclusions by direct observations on the different silicates contained in the pyroxenes, but deduced them from observations made by M. Mitscherlich on different substances obtained separately by chemical processes and compounds of different bases combined with the same acid." M. Mitscherlich has quoted three crystallized substances found in nature as analogous to those he obtained artificially, namely, the sulphates of lead, baryta, and strontita. "These analogous compounds," observes M. Haüy, "of three bases combined with the same acid, should have the same primitive form, and M. Mitscherlich without doubt has examined closely into the matter to satisfy himself if this example be favourable to his views. The fact is obviously otherwise. The primitive form of sulphate of lead is a rectangular octohedron, and consequently incompatible with that of sulphate of baryta and sulphate of strontita, which is a right rhomboidal prism. Moreover the angles and dimensions of this prism differ obviously in the two species, the angles of the base in the sulphate of baryta being 101° 32′ and 78° 28′, and in sulphate of strontita 104° 28′ and 75° 12′.

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* Traité de Minéralogie, Second Edition, p. 39.

"M. Mitscherlich has not been more fortunate in the identity of form which he fancies he has discovered between two other natural substances, whose composition has nothing in common, namely, sulphate of copper and axinite. The three angles which measure the incidences of the faces of the parallelopipedons, the primitive form of those two substances, are, for the sulphate of copper, the first 124° 2′; the second 128° 37′; and the third is 109° 32′; whilst for axinite two are right angles, and the third is 101° 30′. Such are the contrasts which M. Mitscherlich takes for characters of identity."

The form of the crystals of sulphate of magnesia and sulphate of zinc is, according to Haüy, a right prism with a square base, terminated very commonly by a right quadrangular pyramid. M. Mitscherlich quotes them as "another example, but the angle," says Haüy, "formed by two of the faces of the pyramid taken on two opposite sides is about 10° greater in the sulphate of magnesia than in the sulphate of zinc.

"Moreover, how is it that the results announced by M. Mitscherlich, are on every side, in contradiction to those presented by natural productions, as if affinity played a different part in his laboratory to that which it acts in the laboratory of nature?

"Take a view of the various crystals that are found in our cabinets containing different bases united to the same acid, and throughout their geometrical forms will be seen to differ more or less. The primitive form of phosphate of lime is a regular hexahedral prism, that of phosphate of lead a rhomboid, that of phosphate of iron an oblique rectangular prism, that of phosphate of copper a rectangular octohedron, and that of phosphate of manganese a rectangular parallelopipedon. If we take the muriates, the primitive form of muriate of ammonia is a regular octohedron, that of silver a rectangular parallelopipedon, that of iron a right rhomboidal prism, and that of copper a rectangular octohedron; and so of the rest."

It does not appear that M. Haüy has taken into consideration the difference that water, chemically combined, may produce in crystalline forms; at least he says nothing about it in his arguments against isomorphism in the passages we have quoted. He continues thus:—"According to these observations, if the opinion of MM. Berzelius and Mitscherlich with regard to pyroxene, a natural mineral, be correct, it follows that its constitution is an exception to the general results of the crystallization of natural bodies, and appears to be inexplicable.

"I must add, that on the preceding idea, it would be very difficult to form a clear idea of what constitutes the species, pyroxene, in a chemical point of view. The different silicates which occur as constituent parts of that mineral have nothing fixed, either in respect to their number in the same individual, nor in their proportions. Supposing all the combinations of which they are capable, taken one and one, two and two, three and three, to exist in nature, we shall have fifteen different modifications of pyroxene; and if we reflect that in the analyses hitherto made of different pyroxenes, the quantity of magnesia varies from 4·5 per cent. to 30. that of iron from 1·08 to 17·38, and that of manganese from 0·09 to 3, what a series of shades shall we obtain if we multiply those analyses!"

According to Haüy's views, all the pyroxenes contain a common basis of elementary molecules, which determines their true composition, and by a necessary consequence the invariable form of their integrant molecules, and all the other ingredients, which he considers as purely accidental, are only interposed amongst the molecules of the essential substance without affecting its characteristic form. That substance he assumes to be silicate of lime, for in fourteen analyses the quantity of lime was nearly constant, and in the proportion of about 20 per cent. on the whole mass. "I do not know," says he, "why M. Berzelius has supposed that it may replaced by magnesia; how can it yield a place to that substance which it has never abandoned?"

More lately Mr Brooke has also questioned the stability of this hypothesis,* and has asserted (as we have seen that

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* Edinburgh Philosophical Journal, vol. xii. p. 12.

Haüy had done before); that the supposed identity of isomorphous bases does not exist, and that the *apparently* similar forms belonging to substances which differ in composition, do *really differ* from each other in measurement, although in some cases by only so small a quantity as not to be appreciable by the goniometer. Mr Brooke remarks, that "the instances which M. Mitscherlich has adduced in support of his theory, or we may almost say as its foundation, are not in accordance with it;" and he then goes on to show the differences in the inclinations of the planes, in the sulphates of lead, baryta and strontita. "These," he adds, "are *natural* crystals, and evidently do not support our author's theory."

Mr Brooke then states, that the artificial salts of those three bases accord with it no better, and that the *acetates* present even much greater discordances than the sulphates. "The theory is not better supported by the carbonates of lime, iron, and zinc, which are stated to be isomorphous. The primary forms of these substances are rhomboids, and the inclination of P on P′ has been ascertained to be as follows:—

Carbonate of lime	105°	5
iron	107	00
zinc	107	40

Mr Brooke adds, that he is informed that the theory on more mature consideration has been abandoned by the author himself. If that be so, his candor reflects the highest honour on M. Mitscherlich, whilst the necessity of relinquishing a favourite hypothesis furnishes an additional argument against the adoption of those dogmas which, in some measure at least, led to its original formation. For, if Mr Brooke's information be correct, we must object to them, not merely their negative quality of uselessness, but their positively mischievous tendency to induce or confirm error. But, to return to our original subject, however that may be, the assumption that the stronger bases must contain more than one atom of oxygen, should be established on a better foundation than mere analogy, or such arguments as we have met with in the preceding pages, before it is made the ground-work for superseding the beautiful simplicity of the atomic theory as promulgated by Dalton, and substituting in its stead the unnecessary intricacies introduced by Berzelius.

We are not, however, surprised that the hypothesis should have made considerable progress amongst our fellow chemists on the Continent. Its ingenious promulgator has, we believe, a large number of pupils, and it is perfectly natural that bred in his school they should warmly support and propagate the doctrines of so admirable a master; for in most respects, few chemists in Europe deserve that epithet more justly than Berzelius. The accuracy of his analyses, the incomparable ingenuity which many of them demonstrate, the indefatigable ardour with which he pursues his darling science, and the multitude of important facts with which his genius and industry have enriched it, give him a high claim to the admiration of every chemist in the world. It does not follow, however, that he is therefore infallible, and after the best attention we have been able to bestow on his peculiar modifications of the atomic theory, we see no reason for preferring them to the simpler doctrines taught in England, and until they shall be fully convinced of their superiority by *facts* derived from *experiment*, we hope the great masters of our own schools will adhere to their present system, both in their lectures and their publications.

Art. XXXVII.—*Some Remarks respecting the Utility of Chain Cables.* In a letter from Captain Basil Hall, F. R. S. to Professor Jameson.—[*Edin. Phil. Jour.*]

Although I cannot give you by any means so complete an account, as you require, of the admirable invention of chain cables, I shall be happy to extract from notes which I have made, from time to time, a few particulars calculated to give you the information you want.

In the first place there is no doubt that Captain Samuel Brown is the inventor of the chain-cable, and that he is the person who has the merit of proposing its introduction in the Navy. In January 1808, Captain Brown suggested the advantage which would arise from employing iron-cables as well as iron-rigging. In February of the same year, he took out a patent for this invention. About the same time he went to the West Indies in a ship in a great part rigged with iron, and fitted with cables of the description he had proposed for general adoption. The result of this experiment was considered so satisfactory, by a committee of naval officers directed to inquire into the subject, that two line-of-battle ships, a frigate, and a sloop of war, were ordered to be supplied with chains of 100 fathoms length. In 1811, several frigates and sloops were fitted in like manner, and the success which attended these early experiments, though, from prejudice and ignorance, attended with occasional difficulty, gave reason to believe, that, in process of time, the substitution of iron for hempen cables would become, if not universal, at least nearly so. In 1812, Captain Brown devised a new method of closing the links at the side, by means of a long scarf, which rendered the welding more secure; and it has been found, since that time, that when these cables are exposed to a strain, sufficiently great to break them, they do not give way at the point where the links have been welded in this manner. Captain Brown adopted various forms in the links of his cables: at first they were twisted, and without what is called a stay-pin, or internal support across the middle of its length. In the same year Captain Brown availed himself of the powerful aid of the hydrostatic-press, and devised a proving machine, by which every cable, before leaving the manufactory, was subjected to a severe test; and since the introduction of this contrivance, the accidents, formerly complained of, have very rarely been heard of. So far there is no dispute as to the originality of Captain Brown's inventions. He obtains accordingly full credit for them, and certainly deserves the gratitude of the country. What may be still more satisfactory to hear, perhaps, he receives from every corner of the globe the heartiest thanks of the practical seaman, from whose mind this admirable invention takes away a thousand anxieties, and gives him rest and security, at moments when the horrors of shipwreck would stare him in the face. No one, indeed, but a person who has navigated on rocky and stormy coasts, or even in milder regions, but where the ground is thickly set with coral reefs, utterly destructive to hempen-cables, but which make no impression on the chain, can have any conception of the superiority of the one over the other. It is often absolutely indispensable to come to an anchor; and there are numberless occasions when the safety of the ship and crew depends wholly upon the cable not breaking; no one, I repeat, but a sailor, can enter fully into the painful anxiety of such moments, or judge of the fatigue of watching through the long dark nights of high latitudes, for the fatal event, hanging on a few threads, perhaps worn and rotten, which is instantly to decide the fate of all on board. Captain Brown's chain never wears, never rots, is never cut by coral reefs, is always fresh and efficient, and is, perhaps, the greatest boon given to seamanship in modern times.

It was not to be supposed, however, that an invention of such extensive practical utility would be allowed, in this country, to remain in the hands of one man, or that the ingenuity of others would not improve upon the original idea. Accordingly, early in 1813, Messrs Brunton and Company took out a patent for an improved chain-cable. This improvement was said to consist in having the links formed in one plane, in contradistinction to being twisted, with the important addition of a stay-pin across the link, not sharp pointed, but broad at the ends. These are the description of links now universally manufactured by Messrs Brunton and Company, as well as by Captain Brown and others; nor does it appear that there is left any thing further to desire on this branch of the subject.

The originality of this very great improvement is claimed for Captain Brown, by Mr Knowles of the Navy Office, in his published work, "An Inquiry into the means which have been taken to preserve the British Navy." This statement led, in 1821, to an angry controversy between Mr Knowles and the Patentees alluded to. This controversy does not leave the question very well determined, though it might certainly have been settled with a tenth part of the writing, by an appeal to facts, which must be on record somewhere. At all events, whichever party be right in this particular, the great honour due to the original discoverer belongs indisputably to Captain Brown.

The following list shews the size of the chain-cables supplied to his Majesty's ships.

Size [circumference] of Hempen-Cable.		Diameter of Iron forming the links of the Chain-Cable, considered equivalent to, and supplied instead of, Hemp.
25	to 23 Inches.	2⅛ Inches.
22	21	2
20	18½	1⅞
18	17½	1¾
17	16½	1⅝
16	15	1½
14½	14	1⅜
13½	13	1¼
12½	11	1⅛
10½	10	1
9½	9	⅞
8½	7½	¾
7	6½	$\frac{11}{16}$
6	5	⅝

150 fathoms, or 1½ cable, are allowed to all ships of the line; 200 fathoms, or 2 cables, to frigates, sloops and brigs: but small vessels, with complements under 60 men, are supplied wholly with iron-cables.

The method of using chain-cables in place of hempen ones, is daily better understood; and their great value generally acknowledged, even by those who have not acquired a thorough knowledge of all their merits. No ship, now-a-days, would be considered safe without at least one such cable. They are exceedingly convenient in practice, and are applicable, with certain modifications, to almost every variety of circumstance. If once duly put to the proof, they may be relied upon, ever afterwards, with confidence; and in this property consists their chief excellence over hemp. The value of an ordinary cable decreases with its age, even supposing it not to be in use. But if it be used at all, even in the least destructive cases, in fine weather, and in good anchoring ground, still it infallibly deteriotes;—the mere weting and drying,—the necessary wearing in the hause-hole,—the friction of the messenger and nippers, are all inevitable sources of destruction, which no care can exempt it from. Against all these evils, the chain is by its nature entirely free. This being the case, the method of estimating the relative strength of the two, by fixing a new cable to a new chain, and straining them till one or the other breaks is unfair. Let an old chain and an old hempen-cable be tried together, and what will be the result? In point of fact, no such trials can ever afford a fair comparison; because the wear and tear of no two hempen-cables is alike. One may be in use six weeks, and be scarcely the worse; while another may be rendered useless in six days, or in six hours; under circumstances which no degree of professional skill can foresee, or guard against. While under the very same circumstances, the chain, if it stand the trial at all, may fairly be described as being better after the experiment than before; inasmuch as the discipline it has undergone shews its powers. To ships cruising in the Channel, within a few hours sail of a dock-yard, this consideration may be of little consequence; for as soon as a cable is chafed, a new one may be procured. But on foreign stations, especially those without dock-yards, or where, as in South America, the stores required are not to be procured at any cost, the question becomes a vital one. There are many enterprises of the greatest pith and moment, which cannot be undertaken if the ground-tackling be not trust-worthy; and innumerable commercial speculations are put to imminent hazard, or altogether prevented, by the absence of secure means of anchoring at all seasons, and on all descriptions of coast.

In an economical point of view, the advantage to the country in the equipment of his Majesty's ships is very great. As an example, I may state what came under my own knowledge on a recent voyage which I made to South America. We sailed in his Majesty's ship Conway, from Portsmouth in July, 1820, and returned to England in the beginning of 1823, having been in commission somewhat more than two years and a half. In this interval, the ship visited thirty-six sea-ports and roadsteads, and many of them repeatedly; besides which, there was a good deal of coasting, and river navigation; upon the whole, there was probably more than an ordinary proportion of anchoring than is usual in the same length of period. Judging from experience in similar climates, I should say that six new cables of the best description of hemp, would have been completely worn out by the same service, the cost of which could not have been less than £650. But in consequence of our having an iron-cable to perform the greater part of the work, a single hempen one in addition was found quite sufficient. This cable as may be supposed, was completely worn out; while the chain, costing originally less than £200, was returned into store as good as ever, with the advantage of having been well tried during the voyage. Had I been ordered to re-equip the ship for service, I should certainly have applied for this old chain in preference to a new one.

During the voyage in question, it often became advantageous to the public service to anchor in particular parts of a harbour, where the bottom was rocky, and in situations where the best hempen-cable would have been chafed through in a single tide. At others, we were enabled to lie in security on open coasts, which, previous to the introduction of chain-cables, no ship dared to approach. I have frequently, also, seen merchant ships trading on such coasts to great purpose, when other vessels, equally well circumstanced in other respects, were obliged to relinquish considerable mercantile profits for want of this single article in their equipment. Important advantages, therefore, belong to its use besides the saving of cost, which is very great.

Some practical details may perhaps interest you. It is troublesome to use a chain-cable in water above twenty or twenty-five fathoms in depth; for its weight, added to that of the anchor, makes it sometimes a difficult affair, and always a tedious one, to heave it up. Some years ago, when I was inexperienced in the use of the chain, I had occasion to anchor on a coral bank in the China seas, in about thirty-five fathoms water: not reflecting on the momentum which the chain would acquire in descending through such a space, I omitted the proper precautions, and the whole hundred fathoms in length were carried out of the hause-hole with increasing velocity, and a furious noise, till at length, brought up by the clinch, when the jerk shook the ship from end to end; the whole being the work of not many seconds, though causing a commotion truly astounding to all who like myself saw it for the first time. It employed us four hours hard labour before we recovered our anchor. I have not seen any contrivance in our ships for anchoring with the chain-cable in deep water; but on board an American merchant vessel, I took notice of an ingenious device, which was said to answer perfectly, and if so, is well worthy of imitation. To the anchor was shackled in the usual way, a ten-fathom length of chain, to the end of which, by means of three tails, made of small chain, attached to a swivel-ring, was spliced a hempen bower-cable. The iron part of the cable, it was said, lay on the ground, and received the chafe without injury, while the remaining length was kept sufficiently tight to clear the rocks at the bottom. At first sight it might be imagined that hemp and iron would not do well to splice together, but experience in this case shews that it may be done without difficulty, and without injury to the softer material.

This leads to the consideration of another valuable result of experience. When chain-cables were first introduced, it was supposed that a ship could not be moored by their means, unless two were used at once; it being taken for granted that if a ship were moored with a chain and a hempen cable, and that the hause became foul (the technical term for the twisting of the two cables, caused by the ship's swinging round in a tide's way), the iron would speedily cut its companion through. Ample experience, however has shewn that so far from the chain having the supposed tendency, it has it incomparably less than the other. That is to say, two hempen cables drawn across one another, especially when wet (which they must be in a ship's hause), will chafe and wear each other through sooner than a hempen cable similarly exposed to the rubbing of a chain. The reason is, that the links of the chain being smooth, do not act with so much friction. In point of fact, the chain has not so great a disposition to move, for its great weight makes it to lie more steadily across the other.

This valuable principle, which was quite unlooked for in the early stages of the invention, has been in some instances absurdly counteracted by the over-caution of well-intentioned ignorance. It is the universal practice, in using hempen cables, to round them, that is, to wind round them, at those places exposed to friction, a thick coating of small rope, which adds about one-third to the size of the cable,—a clumsy, but indispensable precaution. The friction, therefore, of the two cables, is wasted on the rounding, while the cable itself is protected. Such precaution is clearly not wanted in the case of a chain; but people who act by what is called the "rule of thumb," and who do not inquire into the reason of any thing, think it necessary when they come to moor with one of each kind, to round the iron-cable as well as the hemp; the effect of which is, to convert the smooth and harmless chain into a file of the most biting quality; and whenever a chain so armed comes to lie across a hemp-cable, the destruction of the last is most rapid.

At first, it must be admitted that the iron-cable is difficult to handle, but a little practice renders it wonderfully manageable. It is shackled to the anchor in a tenth part of the time spent in "bending" a cable, and, if required, is unshackled in an instant. In this respect, too, it possesses a property of great practical utility; it takes no injury by being perpetually attached to the anchor,—whereas it is ruinous to a hempen-cable to be long bent or fixed to the anchor, and exposed to the weather. Thus, without trouble, or any risk of injury to the cable, a ship can be kept at all times in a state of readiness for anchoring,—an advantage which practical men well know how to appreciate, and the neglect of which has led to many a shipwreck.

The least expensive method of mooring a ship is with two chains, and the time is probably not far distant when this will be done universally. On the South American station, where the weather was generally fine, a bower-chain to seaward, and the stream-chain towards the shore, was found sufficient, and in this case the wear and tear was nothing. Sir Thomas Hardy, to whom the Navy, and, indeed, the profession generally, are indebted for so many useful inventions, contrived a simple method of mooring with two chains, so that the hawse was never fouled, however many times the ship swung round. This consisted in unshackling the stream-chain from its own part, and again shackling it to one of the swivel-rings, which occur at intervals on the bower-chain. Thus the ship rode, as it were, at moorings, the portion of the bower-chain above the water's edge becoming a bridle; an explanation which will be understood by every practical man. The same officer has also contrived a double bridle for riding a ship by the middle of a cable, or between two bower-cables; and it is probable that all his Majesty's ships will eventually be supplied with these valuable additions to the chain. It may be said that these things are perfectly obvious, and that little credit therefore belongs to the discoverer; but the same is equally true of the chain-cable itself, sufficiently obvious now it is pointed out. But it is the peculiar province of genius to turn those principles to account which ordinary men are trampling under foot. Simplicity, indeed,—which is the most common characteristic of such adaptations,—applies peculiarly to all those of the officer in question, and to none more than to his stopper for the chain-cable, a contrivance not generally known, but meriting a particular description.

On the under side of the beam, which forms the foremost part of the hatchway, where the chain-cable comes up, is fixed a strong crane-necked hook of iron, nearly as thick as a man's wrist, and about as large as the circle which a man can form with his arms when his hands are joined together. One end of this curve is attached to the beam, but is allowed to be moved round the bolt freely in a horizontal plane. The cable is supposed to come up the corner of the hatchway, so that the crane-necked stopper, being placed also near the angle, is made to embrace the chain; and a strong tackle being then hooked to the end of the crane, it is boused or pulled tight on the lower deck, so as to bind the chain closely against the beam. Such is the powerful grasp of this stopper, that it arrests the chain if it be required instantaneously, whatever velocity it may have acquired,—an object which every other description of stopper has failed to accomplish.

There are still, or at least there were two years ago, several desiderata, which it would be well worth the while of the manufacturers to supply. In the *first* place, the mode of unschackling the lengths of the chain requires to be improved. Unless constant attention be paid to the pins which retain the bolts in their places, they become rusted, and are not to be moved without a long process of hammering. Now, since it frequently becomes necessary to slip the cable (for it cannot be cut), the utmost facility should be afforded for disengaging one part of the cable from the other. The fault of the pins seems to lie in their being made flush with the bolt; whereas, were they prevented from coming completely through the hole, by a shoulder, which should correspond with a contraction in the hole, the pin might be reached by a punch, and readily driven out. By the present method, the hammering which is requisite, has often the effect of rivetting it more firmly in its place.

In the *next* place, every ship fitted with chain-cables, ought to have large and strong shackles fitted to the shank of the anchor, in place of the rings used for hempen cables. The short nip of the chain, on these large rings, is apt to break them. I have myself seen no fewer than four rings snapped in this way. I believe that his Majesty's ships are now furnished with shackles such as I describe; but all ships ought to have them.

I have heard, that chain messengers have been introduced; but I have not seen them. It is certainly desirable that they should be; but some contrivance will be necessary, to prevent the frightful accidents which are liable to occur in the event of their breaking. At all events, whether chain-messengers be expedient or not, it seems clear that some contrivance should be sought for, by which the hempen nippers may be done away. They are not only very inefficient, but are very expensive. Something on the principle of the iron-claws, by which great stones are grasped and lifted, might easily be adapted to this purpose. I allude to those pincer-shaped implements, which clasp their object more firmly, the more they are pulled upon. The tails of these nippers might be of the usual kind, especially if the messenger were also of hemp; and I have great doubts about the successful application of iron to this branch of the subject, where the risk of injury to the people employed is so great, and, as I think, unavoidable. It is sufficiently serious when a hemp-messenger breaks, but the breaking of a chain would sweep every man off the deck. It is worthy of the attention of an ingenious man like Captain Brown, to overcome this difficulty; and, perhaps, it may be possible, by some mutual adaptation of the different principles of rope and iron, to place a chain in the middle of the messenger, like the heart in a four-stranded rope, and thus to gain the strength of the one, without losing the friction and the protecting quality of the other.

In reply to your queries respecting the various other uses to which iron has been applied of late years in nautical affairs, I have little to say, as I fear I have already greatly exceeded your limits. I shall, therefore, merely allude to the most remarkable. These are,—spindles for capstans,—shrouds for masts,—bobstays and gammoning for bowsprits,—topsail-sheets and ties, (but this, I believe, is in the merchant service alone, though worthy of imitation in the Navy), oil and varnish barrels, and many minor purposes, all conducing more or less to the efficiency of ships having long voyages to perform. But one of the earliest and still the most important uses of iron, was the four feet cubes or tanks for holding water, a great blessing to the seafaring community. By means of this contrivance, water may be kept for any length of time, without the slightest perceptible contamination. I once filled a tank with clear water at Portsmouth Harbour, and having carried it four times across the torrid zone, and round Cape Horn, over a greater distance than the circuit of the globe, brought it back again more than two years afterwards in the same tank, not in the least degree discoloured, and in all respects as good as when it was first taken up from the spring.

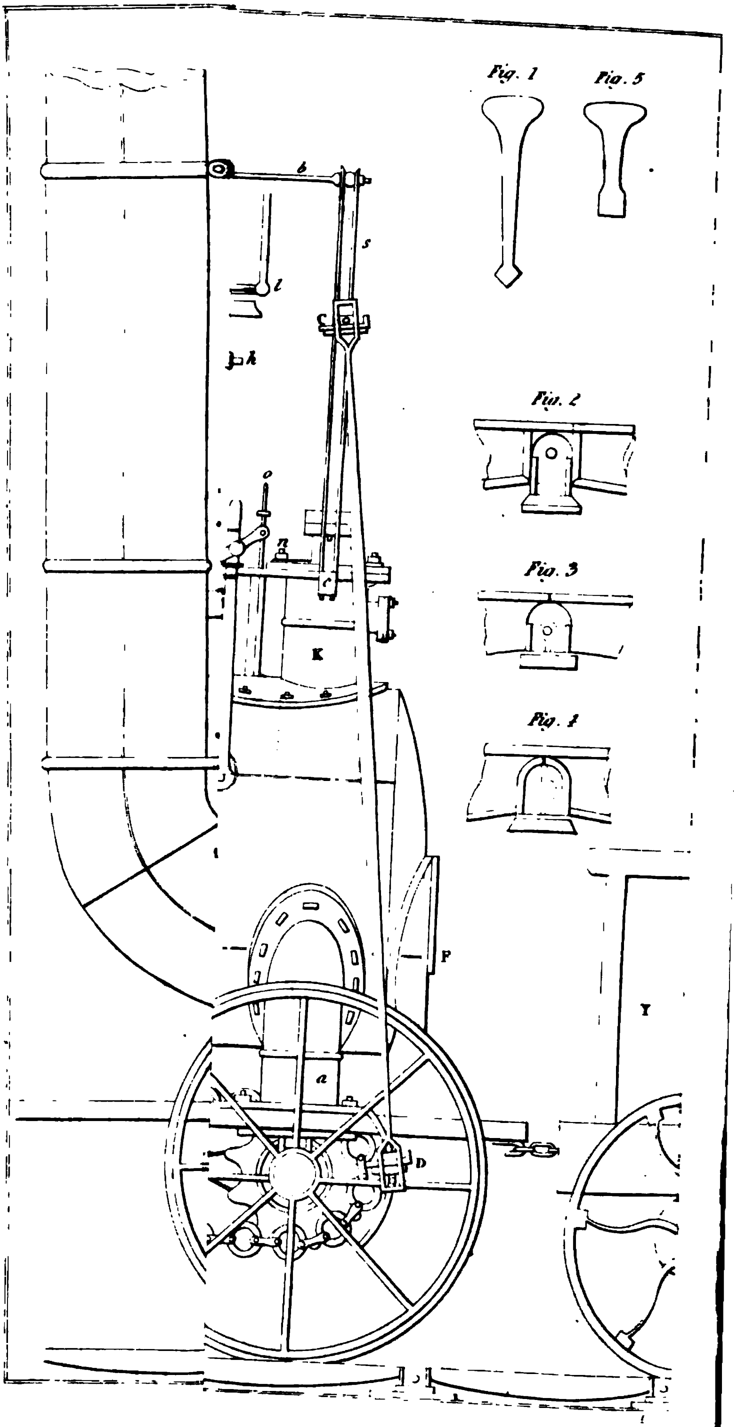
DUNGLASS,
24th Aug. 1825.

ART. XXXVIII.—*An Account of the recent Improvements connected with Rail-Roads; principally compiled from the late Works of Messrs Wood, Tredgold, and Sylvester, upon this subject.*—[T.]

THE late promising application of the steam-engine to transportation on rail-ways, has awakened a spirit of experimental inquiry into the whole subject of inland communication, the beneficial influence of which will not be confined to the age, nor to the country which has produced it. We propose, in this article, to give an account of the late inventions connected with rail-ways, and to examine some of the experiments which have been made, tending to show under what circumstances they are to be preferred to roads or canals.

The iron rail-ways are of two kinds, called the tram or

Pl. II.



TH RAIL ROAD.

plate, and the edge-ways. The general forms designated by these terms are well known. The materials of the tram-way cannot be arranged favourably to strength; but the form of the edge-way, on the contrary, is not only one of the strongest that can be given to a certain quantity of iron preserved in a solid; but there is likewise an advantage in the inflexibility derived from its figure; so that after an examination of the advantages peculiar to each kind of rail-way, the English engineers seem almost unanimously to prefer the edge-way. We shall therefore confine our notice to this kind of rail-way and examine its peculiarities in some detail.

The tracks or wheel-paths of an edge-way, when made of cast-iron and of the most approved construction, are formed of pieces from three to four feet long. A profile of a single piece shows it perfectly straight at the top, but the bottom is of a curved form, the piece being 6 inches deep in the middle and 4 inches near the ends. It does not seem to be yet decided what curve is most fit for this outline of the rail. It is generally made parabolic. The strongest form is semi-elipse; yet Mr Tredgold is of opinion that it would be better to make the under side straight and parallel with the top, for the reason, that the depression occasioned by the load will be less. The advantages of any one of the curves used, over another cannot be great, as they differ but little from each other; but the propriety of determining the fracture to the middle by making the under side straight, may well be doubted.

A cross section made through the middle of a rail, presents an outline slightly convex at the top, and 2¼ inches thick. About 1 inch below the top the sides approach each other to within ½ an inch; they run downwards nearly parallel to each other to within about 1 inch of the bottom, when they swell out again, so as to make the rail, near its under side, more than an inch thick. A section of this rail is shown at Plate II. Fig. 1. A piece of cast-iron formed to this description, if 4 feet long, will weigh about 58 pounds. There seems to be some evidence, from experiments, that rails made of a mixture of different kinds of cast-iron are stronger than if made from a single variety of ore.

It appears from twenty-six experiments, given by Mr Wood, made upon rails 3½ feet long between the supports, 6 inches deep in the middle, 2¼ inches thick at the top, ½ an inch in the middle, and 1¼ inches at the bottom, and weighing 56½ pounds each; that such rails require, upon an average, a load of 14,900 pounds to produce a fracture. None, in these experiments, gave way when loaded with a weight less than 10,668 pounds, and one of them required 23,268 pounds to break it. The rails, on which these experiments were made, were of the size intended for a way on which a load of 4 tons was to be supported in a carriage having four wheels. It was considered that it might sometimes happen, from irregularity in the lines of way, that 2 tons would be thrown upon one wheel. This would be about $\frac{2}{5}$ths of the load required to break the weakest rail subjected to experiment, and Mr Wood, whose experience gives great weight to his opinions, thinks it would be unsafe to load any cast-iron rail-way with more than $\frac{1}{3}$ of the calculated strength of the rails.

One of the greatest obstacles to the extension of rail-ways, has been the difficulty of preserving the tracks in the line in which they are first laid down. Subjected, as the foundations necessarily are, to be undermined by rains, and hoven by frosts, it is no wonder that this yet remains the weakest point of the rail-way system. To prepare a proper foundation for the tracks, Mr Tredgold recommends that, even in firm ground, a trench should be dug under each line, 2 feet wide and 10 inches deep, and be filled up with broken stone, and suitable drains made, to keep the foundation thus prepared, free from water. We do not understand that it has been a common practice in England to prepare for the tracks in so thorough a manner as is here recommended; but we apprehend that a yet more substantial foundation will be required in this country, to guard against the severe effects of our frosts. The use of blocks of stone to support the ends of the rails, appears to be now universal in England. These blocks are at least 16 inches square at the bottom and 8 or 10 inches thick, and it is necessary that the horizontal faces be parallel. The blocks being fixed upon the lines, at distances corresponding with the lengths of the rails, are fit for the reception of what are called the chairs. These are pieces of cast iron, having two projections standing upwards about 3 inches, and far enough apart to receive the ends of the rails between them. The bottoms of the chairs are imbedded into the stones, and they are kept in their places by wooden pins. Many ways have been proposed for uniting the rails to the chairs. One, which seems to be highly approved, is, to have the sides of the rails near their ends, tapered to a sort of wedge form, so that the ends may be made to over-lap each other about three inches, without increasing the thickness of the track. A hole is then drilled through the upward projections of the chair and through the two rails, where they over-lap each other, and the rails being put in their place, between the projections upon the chair, a single iron pin is put through the whole. The pin is not intended to bear the weight of the load; but merely to keep the rails in their proper situations, binding together the whole line. The bottoms of the rails, under the laps, rest upon the chair, which is formed into a segment of a cylinder, the axis of which runs transversely to the rail tracks. If therefore, by the yielding of the foundations, the chair becomes inclined from the horizontal direction, the support of the two adjoining rails will still continue to be the same transverse line, and the ends must remain of an equal height; thus preserving the uniformity of the level. This method is shown at Fig. 2.

The method here described, although more approved than any other, is not considered so perfect as could be desired. For it will be perceived that the chair can move only round the pin as a centre, and when the pin is made to fit the holes, as it is desirable it should do, and the bottom of the rail rests upon the chair, the form of these parts does not permit this motion freely to take place; but it is constrained to it by the load passing over it; the pin is by this means loosened, and the whole joint injured.

Two other methods of laying the rails, in the chairs, are noticed by Mr Wood. In one of these, the ends of the rails are made square, with the exception of a semicircular indentation, and they are placed in the chair, abutting each other. The adjoining indentations form a round hole, through which a pin is passed, securing the rails to the chair by passing likewise through the projections; as in the preceding method. In the other form, the pin is wholly dispensed with, by producing, upon one end of each rail, a small projection which fits an indentation in the end of the rail abutting it. Neither of these methods tie the rails together, in the direction of their length, and any yielding of the foundation, causing a displacement of the chair, destroys the continuity of the level. The part of the chairs on which rails of this kind rest, is made a plane surface. Both of these methods will be understood by referring to Figures 3 and 4.

Although bars of wrought iron had been used for rail-ways more than twenty years since, it was not until 1820, that, in consequence of an improvement in their manufacture, by Mr Birkinshaw, they became used to much extent. The improvement consists in a method of rolling them somewhat near to the proportions of the cast-iron edge-rail; the form of which is taken as the model for imitation. This, as will readily be conceived, is performed by indented rollers, and from the figures of them, given by Mr Wood, they seem to have succeeded in putting the iron into a form, nearly resembling the cast-iron rail. (Fig. 5.) They are rolled into pieces about 18 feet long. Each piece has the appearance of five pieces of cast-iron rail, joined, by their ends, into a single length. They are imposed upon the foundation, through the intervention of stone blocks and chairs precisely similar to those used under the cast-iron rails. The lap-joint, however, is not used; but the ends are made to abut each other. It will be understood that each piece, such as is here described, requires 5 chairs, and is supported as though it were 5 separate pieces. A great advantage, arising from the length of the pieces, is, that the abrupt distortions, to which all rail-ways are subject at the joints, are in these, necessarily reduced in number, if not diminished in degree.

Wrought-iron rails are not subject to be broken; as the load which can be permitted to pass over them, must necessarily be kept below that, which would produce a *set* or permanent curvature in the rail. This is a consideration of vast importance, for in rapid travelling, such travelling as will be introduced upon rail-ways, the breaking of a rail would be attended with consequences not less destructive, than the bursting of a boiler on board a steam-boat.

The experiments of Mr Wood show, that wrought-iron rails, $3\frac{7}{16}$ inches deep, in the middle, $2\frac{1}{4}$ inches at the supports, $2\frac{1}{4}$ thick at the top, and $\frac{5}{8}$ths in the body; the length, between the supports, being three feet, and the weight 28 pounds; will bear about 14,728 pounds, without producing a permanent bend in the rail. The deflection of the rail under this load, was .57 inch, and it did not recover its original shape, if bent much more than this. Wrought-iron rails of this size, are considered sufficient to bear carriages, which, with their load, shall weigh 4 tons; and the rails, on the whole, are taken as equivalent to the cast-iron rails which we have before noticed. Much diversity of opinion prevails, amongst engineers, on the question of general superiority of cast, or wrought-iron rail-ways. The difficulty seems to be, that the facts, in the case of wrought-iron rails, are not yet well ascertained; and until these shall be fixed by experience, opinions must fluctuate and predictions oppose each other.

An opinion is very prevalent in this country, that beams or strong pieces of wood, with thin wrought-iron tracks upon their faces, will form rail-ways more suited to our circumstances, than those made wholly of iron. We apprehend that the opinion is unsound. The cost of building such a rail-way will approach more nearly to that of building one of iron than is generally imagined. The force of traction upon it, we have good reason to believe, will be nearly double; to say nothing of its being half of the time impassable for want of repairs, and we know not how much of the time from undergoing them. An argument often urged in favour of wood is, that a rail-way in this country would be an experiment and that it could be thus more cheaply made than with iron. A rail-way upon any route, of which the traffic is unknown, would be an experiment. But the rail-way system is no longer experimental; and our researches and proposed discoveries in this branch of art, have long been anticipated; so that we cannot promise to ourselves the honours of invention, as a reward for our experiment.

Several kinds of turning platforms have been devised, to permit of carriages to pass each other, when on the same rail-way and going in opposite directions, or with different velocities, in the same direction. It may be unreasonable to expect that any device of this kind will answer the end so well as two entire rail-ways; one for the carriages passing in one direction and another for those running in an opposite course. But even with two sets of tracks, the turning platforms are necessary, when the route is a long one, to permit the swift carriages, carrying passengers, to pass the common convoy wagons; and this, we believe, is the practice intended in England. But on the great, projected rail-way between Manchester and Liverpool, it is proposed to have four complete carriage-ways; to provide for the different velocities, as well as the different directions of the carriages.

At an early period of the English rail-ways, the carriages were mounted upon cast-iron wheels. It is evident that such wheels make less resistance than those made of wood, from possessing the property of preserving their circular form, against the force of the load, which constantly tends to flatten their lower limbs. It was soon found, however, that wheels cast in the common sand moulds were rapidly worn upon their peripheries. The wheels now used are cast in a hollow iron cylinder, with which the metal, which forms the circumference of the wheel, comes in contact when in a liquid state; precisely in the manner in which rollers, for plating iron, are cast. This method of casting renders the wheels nearly as hard as a file. The effect is generally supposed to be produced, from the metallic mould conducting the heat from the liquid metal, much faster than is done by sand, or any earthy substance. As the rims, cast in this manner, shrink very much in the moulds, it has been found impracticable to form a sound wheel of a single casting. But engineers have surmounted this difficulty by forming spokes of wrought-iron, which are first fixed to the nave, that being cast upon them. They are then laid into the mould prepared for the rim, into which their ends project a little, in a dovetail form. The rim is then cast, and in shrinking, binds the whole together with great firmness. It is usual to make the spokes a little crooked; somewhat in the form of an S. A slight flanch is made to project beyond the face of the rim, which comes in contact with the rail and directs the wheel upon the top of it. The face of the rim or bearing part of the circumference is made a little conical, the greatest diameter being upon the inside. This is to give the wheels a tendency to slide inwards and thus keep the flanches from rubbing against the rails. In order to produce this effect fully, however, it would seem that the outer part of the periphery should be cylindrical, otherwise the two cones press equally in opposite directions, and the tendency of either flanch towards the rail is not destroyed. The wheels do not revolve upon the axles, but are fixed firmly upon them, the axles themselves revolving in boxes, or chairs, as they are called, fixed to the under side of the carriage frame. The wagon bodies, in which goods are placed for transportation, whether drawn by horses or the steam engine, have no peculiarities worth noticing. They are simple square boxes and of capacities suited to the traffic of the place.

Before noticing the resistance necessary to be overcome upon a rail-way, it may not be unappropriate, slightly to examine the resistance which a common road presents to the passage of wheel carriages.

It was once very common to talk and to write of the friction of wheel carriages upon roads, as the principal obstacle to be overcome by the moving force. But a small part of the resistance, however, is from friction of any kind. On the contrary it is the yielding, or breaking down, of the surface of the road, that destroys the greater part of the force applied to the carriage. Animals are worn down, not so much in overcoming the friction of the wheels upon their axles, or upon the road, as in forming ruts and dust. Hence, paved roads, although more uneven, occasion less resistance than common gravel ways. Carriages move yet more easily over roads, the wheel tracks of which are made of large blocks of stone with plane faces. Some of the Roman roads were formed in this way, and the streets of some of the Italian cities are made in this manner at the present time. We can conceive of no better form, except the iron track.

The resistance to carriages on common roads is too variable to admit of any general expression in the form of a law. It may be well enough, however, to notice that it arises from the friction of the wheels upon the axles, from the force which the wheels and the surface of the road, or the materials of which it is composed, are capable of exerting to preserve their form, when that force is less than the weight of the carriage; and from raising the load over asperities of an unyielding nature. The falling of the carriage, however, after having surmounted asperities of the last kind, is auxiliary to the moving force.

The resistance to the passage of boats upon canals arises, almost wholly, from displacing the water, and it is well known that the resistance at different velocities, is very nearly as the squares of those velocities.

Carriages, running upon well constructed iron rail-ways, are resisted by the friction of the wheels upon their axles and upon the rails, by the *inertia* of the load, and by the air; the resistance from all other causes being too small to be taken into the account. Of the three elements here specified, as forming the resistance, one, the *inertia* of the load, vanishes altogether after the required velocity is obtained. It forms, therefore, but a small proportion of the resistance to passing a load any considerable distance; and the resistance of the air, under ordinary circumstances, is hardly appreciable. Both of these resistances, then, are things of curiosity in researches of this nature, rather than serious impediments to the motion of carriages. As friction, therefore, constitutes the principal resistance to be overcome, it has been considered of great importance to investigate thoroughly its laws, so as to place it within the reach of calculation to determine the amount of resistance to any load under any given conditions.

Experiments have been made by some very judicious engineers, upon different kinds of rail-ways to ascertain with accuracy the relations of the rail-way, the carriage, the load, and the velocity to the resistance, and it is satisfactory to observe, that in general, the conclusions of Vince and Coulomb, regarding the laws of the resistance from friction, have been confirmed. It appears to have been established, that the resistance to the same load, is always the same, with whatever velocity it is moved. As the superiority of rail-ways to canals must rest mainly upon this fact, we shall relate some of the experiments which seem distinctly to prove it. Messrs Wood and Stevenson made a dynamometer, the index of which was drawn to the first point of its graduation by a weight. This was affixed to a carriage which, with its load, weighed 8456 lbs. The carriage was drawn by men, on an edge rail-way, through the medium of the dynamometer, and various velocities were given it from 134 to 397 feet in a minute; yet the dynamometer always indicated a resistance of 56 pounds. By another kind of contrivance, by Mr Roberts, the velocity was varied from the rate of 2, to that of 22 miles in an hour; yet the dynamometer always indicated the same resistance. In a great number of experiments made by suffering carriages to run down an inclined rail-way, the spaces passed over in different times were found to be to each other as the squares of those times respectively; thus proving that the friction was a force as constant in retarding, as the gravity of the carriage was constant in accelerating the motion.

Nothing, therefore, seems better established, than that the resistance from friction is uniform and wholly uninfluenced by the velocity of the moving body. It must not be concluded from this, however, that it requires as little expense of force to move a load against this resistance during a given time, at a high, as at a low velocity, or that a carriage may be moved two miles an hour, as easily as one mile. There has been considerable misconception upon this subject, and we apprehend that it has arisen from neglecting to consider the velocity with which a force is applied, or suffered to act, as an element in measuring the effect of such force; or that the forces of the different points of any machine, however driven, are inversely as the velocities of those points. Bearing this law in mind, we shall perceive, that although a resistance, as friction, be uniform at all velocities, any force, which we can apply, to overcome it, depends upon the velocity with which it is made to act upon it, and is always in a proportion inverse to that velocity. The velocity of animals, as we shall have occasion hereafter to notice, cannot be increased without diminishing their force yet more than would be assigned by the simple inverse proportion.

We have seen that the amount of the friction is the same in all velocities and it was likewise long ago established, by Coulomb, that the friction, of the same rubbing surfaces, is in direct proportion to the load, without any relation to the size of the rubbing surfaces. The force required to overcome the friction at the axle of a carriage, however, is very much influenced by the proportion which the diameter of the axle bears to that of the wheel. The principal benefit of wheels, when used in passing over smooth and level surfaces, is in giving a mechanical advantage by the lever, into which the wheel may be resolved, to overcome the friction. We shall conceive of this more easily by supposing the carriage to be fixed and the plane, as a rail-way, to move. The relative motions of the two bodies in this arrangement are the same as though the carriage moved over the plane. Then, if we take the centre of the axle as the fulcrum of a lever, and the radius of the wheel, which is directed to the rail-way, as one of its arms, to which the force is applied, and consider the resistance acting on the circumference of the axle, as collected into any point on that circumference, we have the other arm of the lever, equal in length to the radius of the axle. Thus the mechanical advantage obtained by wheels, in overcoming the friction, is in the proportion of the radius of the wheels, to that of the axles; and it follows from this, connected with what we have previously stated, regarding friction, that if the friction were the only obstruction to be overcome by the moving force, the velocity of a carriage, driven by a force of a given magnitude, would become directly proportional to the diameter of the wheels, the axles remaining of the same size.

Mr Tredgold made some experiments with a little carriage having two sets of wheels, one of 8 and one of 4 inches diameter, from which he concluded that the resistance is almost wholly confined to the axles, when the rails are smooth and clean, and that, all other things being equal, an increase in the diameter of the wheels is attended with a corresponding increase in the velocity of the carriage. The experiments of Mr Wood, made with a view to decide this question, are worth relating. He attached to a locomotive engine, having wheels of 3 feet diameter, a train of carriages weighing 731¼ cwt. These were moved, upon a rail-way, a distance of 36 miles in 9 hours and 35 minutes; during which time the engine required 2534 pounds of coals and 890 gallons of water evaporated under a pressure of fifty pounds per inch. The wheels of the engine carriage were then taken off and others, of 4 feet diameter, substituted in their place. The same consumption of coals and water then carried the same load over 48·8 miles; thus increasing the effect rather more than in the simple ratio of the diameters of the wheels. The strongest objection to these experiments is, that they prove too much, for the wheels of the carriages connected with the engine were in both experiments the same; this part of the resistance therefore, remained the same, while the velocity was increased in the direct ratio of the increase of the diameter of the engine wheels alone. Mr Wood seems to be somewhat aware of this difficulty, and attempts to explain it; his explanation, however, is not satisfactory.

The resistance from friction when the rubbing surfaces are favourable, such as iron upon brass and well smeared with oil, was found by Coulomb to be about equal to 11 per cent. of the load. From the experiments made by Mr Tredgold, with his little carriage, it is deduced that the friction amounts to a resistance equivalent to raising $\frac{1}{8}$ of the weight, directly upwards. But the experiments of Mr Wood, made with large carriages fully loaded, give the resistance of friction as equal to about $\frac{1}{14}$ of the load. This is the resistance which would be opposed to the motion, if the load were to slide upon a plane, polished and oiled as perfectly as the boxes in which the axles of the carriage wheels revolve. But as axles $2\frac{3}{4}$ inches in diameter, are strong enough to sustain a load as great as ordinary rail-ways will bear, by using upon such axles, wheels 3 feet in diameter, the resistence is reduced to $\frac{1}{144}$ of the sum that it would amount to, if the carriage were to slide directly upon a smooth and oiled rail-way. From this we see, that upon a good rail-way, and with wheels and axles of the proportions here stated, the resistance is but $\frac{1}{14}$ of $\frac{1}{144}$; equal to about $\frac{1}{183}$ of the weight of the load, drawn directly upwards. The resistance upon a common road may be taken as about equal to $\frac{1}{13}$ of the load. The resistance upon a canal is found, as we have before observed, to vary, nearly as the square of the velocity; it is equal to $\frac{1}{183}$ part of the weight of the floating mass, when the velocity is about $4\frac{1}{3}$ miles an hour. This speed, however, it should be remarked, is not that required for a maximum effect, to produce which, the velocity should be the least possible; as any load may be quadrupled, and moved at half the former velocity, by a given force of traction. But the end of transportation could not be answered if this fact were followed; and many circumstances make it necessary, that canal boats should not move slower than 2½ miles an hour. At this rate of motion, the resistance is equal to about $\frac{1}{310}$ of the load, and with such a velocity the effect of a given force, upon a canal, is about three times that of the same force, upon a rail-way.

The only forces applied to move carriages upon level rail-ways or those differing but little from that direction, are horses and the steam-engine. In some cases, where the rail-ways are short, much inclined, and the traffic descending, the loaded carriages are made to drag up a set of empty ones. We shall confine ourselves, however, to the consideration of such forces as are suited to the purposes of general intercourse.

The mode of applying horses, to carriages, upon rail-ways differs in no essential degree from that common upon other roads. The horse is harnessed to a carriage, and as few rail-ways are calculated to bear more than 5 tons, distributed upon four wheels, if the way be level, two or three more carriages, with like loads, are so attached as to be drawn after the first carriage.

The force of a horse, as it is well known, has been estimated differently by those who have examined it. Dr Desaguliers considered it sufficient to raise 27,500 pounds 1 foot high in a minute. Mr Smeaton took the equivalent weight at 22,916 pounds; Mr Watt 32,000 pounds; and M. Schulze at about 60,000 pounds.* All the numbers here given, seem, from the actual performance of horses upon rail-ways, too high. At the Backworth colliery edge-road, the performance of the horses, as given by Mr Wood, is equal to 24,000 pounds, raised one foot high in a minute. But at the Killingworth colliery it amounts to but 18,200 pounds. All these horses, he says, are very large and powerful. The space they travel through in a day is about 19 miles, and the mean resistance, in the first instance, is 115 pounds, and in the last, 109 pounds to a horse. The rail-ways are a series of planes, varying considerably in inclination, so that in some of the descents the resistance of the carriages is negative; while at one of the ascents it amounts to 314 pounds to each horse. From this circumstance the load is not such as has been assigned for a maximum effect; yet this is in some degree counterbalanced by the change of position affording relief to the animal.

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* An adventitious importance is given to the fact involved in this case, from the circumstance, that the horse power has been taken as the standard measure for other forces. When so applied, the number assigned by Mr Watt is, we believe, always used, and it is not probable that this quantity will be altered, in this application, however it may be proved to exceed the actual force of the animal. It is a very convenient quantity, to form the unit, and having once become definite, by being the sign for a known weight, raised through a given space, in a given time, any discovery, that the force, from which it was derived, is not its exact equivalent, ought not, and, probably, will not have any more effect to alter it, than discoveries of the same kind, which may have been made relating to the kernel of wheat or the human arm, have had, to alter the grain weight or the lineal yard.

The law of the decrease of the force of animals as the velocity of their motions is increased, is worthy of great attention in connexion with the subject before us. It is well known that an animal can exert its greatest force, when the resistance remains at rest; but here, no effect is produced. Again, when the velocity is increased to a certain degree, the whole effort of the animal is required to keep his own body in motion, and in this condition, likewise, the effect is null. All velocities under this are productive of useful effect, and mathematicians have investigated the subject to assign the actual effects produced at different velocities. Euler was the first to propose a formula for expressing the law of animal force. In this he takes P, to represent the weight which the utmost effort of an animal is just able to overcome; W, a weight with which it may, at any time, be loaded; v, the velocity with which it moves when so loaded; and c, that velocity at which the animal can merely carry its own body. He then takes $W = P\left(1 - \frac{v^2}{c^2}\right)$ as expressing all the conditions required. Professor Playfair has changed this formula to $W = P\left(1 - \frac{v}{c}\right)^2$. This differs considerably from the original, in the effect which it assigns to all useful velocities; but he appears to have much confidence that the expression, as he has given it, is near the truth. Now, if we take the resistance of the greatest load drawn by a horse on the Backworth colliery rail-way, as a value of P; and take the actual value of v, upon the same rail-way, 2½ miles an hour, and assume $c = 12$, we obtain 196 pounds for the value of W; this exceeds the average performance of the horses upon the above rail-way; which is but 115 pounds, and although the variation of the resistance, as we have before said, on this rail-way, is not favourable to a high performance, the formula has the benefit of the low value assigned to P, which is not such as requires the utmost effort of the animal to overcome;

but such as he actually draws through a distance of 330 feet.

Professor Leslie has given a very simple formula for the draught of horses. It is $(12-v)^2$; v denoting the velocity in miles an hour. It assigns precisely the same effect as the last of those before given, if P were taken = 144 pounds; and this produces a very near correspondence to the performance upon rail-ways.

We may perhaps, without any great error, consider the above equation of Professor Playfair, as giving the draught, which a horse is capable of continuing for 20 miles a day; if we take P equal to *half* the weight that the utmost efforts of the animal can overcome.*

Very few horses can carry any load at the rate of 12 miles an hour. Carriages, therefore, cannot be drawn, even on railways, at this velocity, by horses. It is not unlikely that 10 miles is within the reach of travelling. This velocity, however, is much above that proper for a maximum effect; which is produced, or the greatest quantity is moved through a given space, in a given time, when the velocity is about 4 miles an hour.

There are two methods of applying the steam-engine to the movement of carriages. One of these, which was first proposed, we believe, by Mr Edgeworth, is to communicate the motion from fixed engines, by chains, passing along the road, which may be supported upon pulleys, both to prevent their wear, and to avoid, in a great degree, the loss of the force of the engine, in the friction of the chain. This method is not generally expected to prevail. The great length of the chains, extended beyond the control of the superintendents, and the difficulty of crossing common roads by them, are objections which have hitherto prevented its adoption.

Although locomotive engines had been projected by Dr Robison and Mr Watt almost seventy years ago, it does not appear that any such engine was actually built until 1804, when Mr Trevithick put one in operation upon a rail-way in Wales, which travelled at the rate of five miles an hour, and drew after it carriages loaded with ten tons of iron. The experiment, however, does not seem to have been successful, and the failure was attributed to the liability of the wheels to turn upon the rails, the carriage remaining stationary, instead of rolling upon them and bearing the carriage over the plane. To obviate these supposed defects, Mr Blenkinsop, in 1811, built, near Leeds, a rail-way and connected with the rails a rack, which, by means of cog-wheels connected with the carriage and turned by the engine, ensured the progressive motion of the carriage. This apparatus is yet in use, but subsequent experience has shown, that the rack may be dispensed with, on level rail-ways or those having but slight ascents.

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* Mr Wood has given $\frac{224}{v}$; v being the miles an hour, for the power of a horse. It is obviously erroneous, as it assigns an effect at all velocities; whereas, it is evident that at a certain speed, the effect must vanish or become = 0.

The locomotive engines, as now built, are among the most beautiful specimens of modern engineering. Simple as this application of steam may appear on a cursory examination, no one can study its difficulties thoroughly, who will not admire the skill with which they have been combatted, if not wholly subdued.

We have given, Plate II. a figure of the locomotive engine used at the Killingworth rail-way. This figure and the description are copied from Mr Wood's work. The figure represents a side view of the locomotive engine with a portion of its convoy carriage, to the right of which the train of carriages bearing the load are attached.

"The boiler is of malleable iron, cylindrical with hemispherical ends. A cylindrical tube passes through the boiler, within 2 inches of the bottom; in one end of the tube the fire is put, and the other end is terminated by a chimney. The grate, whereon the fire is laid, is placed rather below the middle of the tube, and reaches about 4 feet within it, resting, at the farther end, upon a narrow partition of brick, closing up the lower side of the tube below the bars. F shows the fire door, which at the tube end of the fire closes up the upper side of the tube, thus forcing the air through the fire in the usual manner. The boiler rests upon a square frame of maleable iron, supported by the steam cylinders *a a*, two on each side, 7 inches in diameter. The cylinders K K are placed partly within the boiler, as shown in the drawing; they are 9 inches in diameter, and are lined with sheet copper; the piston rods work through stuffing boxes in the usual way, and are attached to cross beams B and C, sometimes formed of one piece and sometimes of more than one. The rectilinear motion of the piston rod is secured by the guides *s s*, fastened to the projecting arms *c c*, cast upon the top of the cylinder and kept perpendicular by the braces *b b*. The connecting rods B D and C D are fixed upon the ends of the cross beams by joints and at the other end by a pin fixed to one of the spokes of the engine wheels at D D. To strengthen this spoke, a circular piece of metal attaches the two adjoining spokes with it.

" The wheels, as shown in the drawing, are 4 feet in diameter and are made of cast-iron with circular naves, rimmed out to fit the axle, and fastened to it by means of iron keys. The axles are of wrought-iron about 3½ inches in diameter, and are turned perfectly round where the chair comes upon them, and also at the extreme end where they pass into the nave of the wheel.

" The chairs, on which the engine rests upon the axles, are made of brass, 4 inches broad, and reach to the semi-circumference of the axles. They are at liberty to move up and down, to conform to the inequalities of the road. Two of these chairs are fixed upon the frame of the carriage and the other two are moved backwards and forwards by a sliding groove upon the frame to keep the chain sufficiently tight. The frame is made of wrought iron welded together; on this the bearing cylinders are fastened by bolts, with a joint, perfectly steam-tight.

" The steam is communicated from the boiler to the cylinder, through a passage, the area of which is regulated by the sliding lever or handle *o*, which, of course, restricts the quantity and regulates the velocity of the engine. The steam is admitted to the top and bottom of the piston by means of a sliding valve; which, being moved up and down alternately, opens a communication between the top and bottom of the cylinder and the pipe *d d*, that is open into the chimney and turns up within it.

" The steam, after performing its office within the cylinder, is thus thrown into the chimney, and the power with which it issues will be proportionate to the degree of elasticity; and the exit, being directed upwards, accelerates the velocity of the current of heated air accordingly. The action of the steam engine is now so well known, that it will not be necessary to describe the mode by which the rectilinear motion of the piston is converted into the rotary one of the wheels, and the progressive motion of the carriage thereby effected. A slight inspection of the plate will convey to those who have the slightest knowledge of machinery, the manner in which it is done, as nothing can be more simple and effective.

" The sliding, or steam valve is opened and shut, at the proper periods, by the following contrivance (represented at Fig. 7); *a b* is a lever fastened upon and turning round with the axle of the carriage, *b c* is a circular opening in the eccentric circle *d e*, within which a pin, attached to the end of the lever *a b*, is at liberty to move. This eccentric circle is loose upon the axle of the carriage, and is only turned round when the pin, at the end of the lever *a b*, arrives at *b a c*, according to the direction in which it is moving. A circular hoop or strap of iron fits the circumference of the eccentric motion connected to the lever *f g h*, which is moved backwards and forwards as the axle turns round; as this lever is moved, its motion is communicated to the arm *i k*, which is shown also at Fig. 6, and through it by the lever *k l* and rod *l m*, to the cross head *m n* and so to the rod *n o* of the sliding or steam-valve, which, as the carriage is moved forwards, is thus moved up and down to open and shut the communication between the two sides of the cylinder and the boiler, at proper periods. *h* is a screw by which the time of opening the valve can be altered, *p* is the man-hole door, to have access into the interior of the boiler; *v* is the safety-valve loaded with the weight W, corresponding with the pressure of steam, which it is found requisite to retain within the boiler.

"The boiler is supplied with water by means of a small forcing-pump, fixed to the side of the boiler and worked by a rod attached to the cross beam of the engine. The diameter of this pump is very small, so that the quantity of water injected shall not reduce the temperature of the steam, in the boiler, so as to be injurious, and prevent the regular supply of steam to the cylinder. The quantity of water required not being great, it is pumped in at proper intervals, when the draught is easy and when the pressure, for the load of steam, is not required to be very great.

"When the engine is used for travelling, the boiler is inclosed within a wooden covering consisting of thin narrow deals, to prevent the radiation of heat; when this is not done, the wind has a great effect in reducing the temperature, by the rapid abstraction of heat. The water and coals, required for the regular wants of the engine, are carried in the convoy carriage Y, attached to the engine. The size of this and the quantity carried, will, of course, depend upon the length of the stages the engine has to travel, or the convenience of obtaining a supply.

"The train of carriages moved by the engine is most frequently attached to the convoy carriage; but the engine can also drive the carriages in front, by propelling them forward; but this is very liable to drive the carriage off the road, especially when turns or curves in the line of the road occur.

"The endless chain is for keeping the cranks of the engine in the proper angles with respect to each other. Many plans have been devised to obviate the use of this chain, but nothing superior has yet been devised. The locomotion of this engine, is effected by the action of the wheels upon the rail without the aid of any extraneous mechanism, and consists of the hold or adhesion of the surface of the wheels against the surface of the rail. The *rationale* of the action will be very evident by an inspection of the drawing, the power of the steam in the cylinders is employed to turn the wheels of the engine round, by a pin fixed in one of the spokes of the wheels, and acting as a common crank; these wheels resting on the rail, press upon them with a force equal to the weight of the engine divided into the number of wheels; thus, suppose there are four wheels, then the pressure of the periphery of each wheel, upon the surface of the rail, is equal to one-fourth of the weight of the engine. Now it is well known, that when two surfaces are in contact, and subjected to a certain pressure, it requires a certain force to cause them to slide upon each other; that property in bodies to resist sliding is called adhesion of the surfaces. Suppose, then, the power of the cylinders be employed to turn the wheels of the engine round, this property of bodies, in contact, will prevent the wheels of the engine from sliding upon the rail, with a force equal to their weight, or pressure, and they will roll along the surface. If, however, the progressive motion be opposed to a retarding force, greater than the force or powers of adhesion, then the surfaces will slide upon each other; and the rotary motion of the wheels will be continued without effecting a progressive motion of the engine."

Nothing can be more beautiful than the method of keeping the weight of the engine equally divided upon the wheels by means of the steam cylinders. These cylinders are accurately bored and open into the boiler. They have pistons fitted to them, which are connected with the chairs, which rest upon the axles of the wheels. The area of all the pistons is such, that the pressure of the steam, in the boiler, upon their top surfaces, is nearly sufficient to raise the whole boiler and its appendages, upon them. They therefore form springs, the elasticity of which is nearly constant, at whatever place the pistons may be in the cylinders, and thus keep the pressure equally distributed upon the wheels, however the rail-way may deviate from a plane.

The weight of the engine here described, including a supply of water in the boiler, is 16,800 pounds. As the progressive motion depends upon the rolling of the wheels, an effect which results from the *virtual friction*, that is, the friction which would result were a sliding of the wheels upon the rails to be induced, a great weight of the engine, however desirable it would be, from other considerations, to reduce it, is necessary, for were the wheels not pressed forcibly upon the rails, the action of the engine would give them a rotary, while the resistance of the convoy carriages would prevent their progressive motion. It has been shown by Mr Wood that under unfavourable circumstances,—and these alone furnish a safe basis for the calculations in engineering,—the wheels of the locomotive engine will not slide upon the rails, unless the resistance to their rolling is $\frac{1}{25}$th part of the weight which they sustain. Hence, upon a level rail-way, a locomotive engine will drag a train of loaded carriages having 7·3 times the weight of the engine. But when the ascent becomes more than 2° 17′ about $\frac{1}{25}$th of the length, the wheels will slide rather than drag any load after them. These machines, as now constructed, and using steam of an elastic force of 50 pounds to the inch, have a force sufficient to draw, upon a level way, a train after them, of 5 times their own weight, at the rate of 5 miles an hour, and an engine, however great its force, drawing such a relative weight in its train, could not ascend an elevation of even 1°, as the resistance would then become more than $\frac{1}{25}$th of the weight of the engine, and its wheels would slide upon the rails, without progressing. Hence, should the progress of discovery lead to the construction of a steam-engine, in which a given force may be obtained with less weight of machinery and water than are now required, it could not be adopted for locomotion upon rail-ways, unless it were loaded with a weight foreign to itself, or a rack, or some similar contrivance, were united with the rail-way, to ensure its progressive motion; and we believe that the disadvantages of any known apparatus for this purpose, would be greater than those of the present weight of the engine.

The advantages resulting from this application of the steam-engine, although important in Great Britain, particularly in the mining districts, are not, we apprehend, available in New England, from our want of coal.

Mr Tredgold has gone into a sufficiently minute calculation of the comparative cost of horses and of the locomotive engine in England. The result of this calculation is, that the power to transport 1 ton, 1 mile, by horses, will cost 2 farthings. With the locomotive engine 1⅓ farthings. Making the cost, in favour of the engine, as 3 to 2. The price of coals was taken, in this estimate, at 6*d.* sterling a bushel, and a greater effect is assigned to them, than is warranted by experience.

By an experiment of Mr Wood, at the Killingworth railway, he found that 1487 pounds, or 17·4 bushels, of coals were required to carry a train of wagons weighing 81,872 pounds a distance of 26 miles, in 6½ hours. The average price of bituminous coals in New England may be taken at 40 cents a bushel. We have, therefore, for 17·4 bushels $6,96. The interest on the cost of the engine, such as was used by Mr Wood, the repairs and attendance, would not be less than $5 a day, or $2.70 for 6½ hours; making the cost of the work performed, in Mr Wood's experiment, $9,66. Now the work performed at this expense would be within the power of 6 horses, working one day, and would not cost more than $9, allowing 3 men in attendance. We are unacquainted with any accurate experiments tending to show the comparative effects of coals and wood, in producing heat, or forming steam. But we think it very doubtful whether coals would not be found nearly as cheap as wood, in all the populous parts of our sea-coast.*

The cost of transporting 1 ton, 1 mile, at the rate we have given, by horses is $\frac{9}{10}$ths of 1 cent. This embraces no expense but that of the power of moving the load.

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* Since writing the above, we have received Vol. III. Part 1, of the Transactions of the American Philosophical Society, which consists of a paper by Mr Bull, of Philadelphia, giving an account of a very elaborate course of experiments to determine the heat evolved by the combustion of a great variety of substances. The heat produced from wood, compared with bituminous coal, was, in Mr Bull's experiments, greater than we were prepared to expect.

Art. XXXIX.—*Notice of a Work entitled, "Memorial on the Upward Forces of Fluids, and their Applicability to several Arts, Sciences, and Public Improvements: For which a Patent has been granted by the Government of the United States, to the author,* Edmond Charles Genet, *a Citizen of the United States; Member of the Institute of France; of the Royal Antiquarian Society of London; of the Philosophical Society of New York, &c. &c."*

None more than ourselves believe that great discoveries are rare. We mean those discoveries, which, in their consequences, improve the physical condition of man, by supplying his wants with less labour and thus give him a portion of his time, to cultivate his intellectual and moral powers. One of the most sensible questions, in our opinion, that was ever asked regarding the importance of a discovery was, "Will it reduce the price of grain?" The memorial of Mr Genet professes to make us acquainted with one of this character; it professes no less than to add another to the forces, already within the control of the engineer and the manufacturer, which, like the steam-engine, that it is intended to render obsolete, may give motion to the thousand complicated devices of human art. We were not born yesterday, and have therefore had an opportunity of knowing that men oftentimes deceive themselves, sometimes others, into a belief that their discoveries are of greater value than a succeeding age assigns to them. Persons so deceived, however, are, commonly, men of undisciplined minds, men who have not stood before rivals and critics, rising in their despite, to rank with the philosophers and sages of the earth. The author of the book before us, however, is not a nameless man. He is a member of the French Institute, and was, as he informs us, "affiliated to the Academy of Sciences in 1783," before Lavoisier, La Place, or Davy, were known to the world; when, therefore, such a man informs us, that after forty years reflection he is about to enlighten us upon a subject, which of all others he has chosen and kept nearest to his heart, we should be lost to humanity, did we not take up his book with a respectful attention.

"The upward forces of fluids!" What can this mean? but let the author speak. "In my meditations on the homogeneity of the forces usually applied to mechanics, I have viewed with astonishment that the force of levity, or the upwards force, should have been entirely overlooked or neglected, when Newton himself had admitted the existence of a drawing force opposed to the force of gravity, which prevents the moon from falling upon the earth; when Herschel had calculated that the force of levity, as exhibited by caloric, was such that it moved in every direction at the rate of 200,000 miles in a second; when the smoke and the vapours of the earth manifested, by their ascension through the atmosphere, that they were impelled by a force acting inversely to the force of gravity; when that same force, which chemistry has proved to be due to caloric, latent or sensible, was known to be the cause of the ascension of balloons; when those aerostatic machines were seen to raise heavy weights in a different tangent from the force of gravity; when the report of the bold adventurers who raised themselves in the air, by means of those aerostats, had testified that equally independent of the general laws of atmospheric pressure and aerometry, the elasticy of aerostats and the accelerated motion of their ascension, increased as the air grew lighter and the atmospheric pressure decreased; when it was also stated by them, that as they approached the upper strata of the atmosphere, perhaps occupied by an ætherial gas, at the altitude of 3670 rods, the highest to which men ever have ascended, above the geological crust of the globe, the spirit of life itself seemed to be drawn upwards and to become evanescent, retaining no recollection on its return to vital air, of what had happened during its lipothymy, and when finally it had been proved that the laws of gravity varied in the same body in proportion of its degree of heat or cold, as it is evinced under the equator and the colder latitudes towards the poles, by the pendulum, retarded when rendered specifically lighter and bulky *by caloric*, or accelerated when rendered heavier and contracted *by cold*." p. 5.

Leaving out the theory of the variation of the pendulum, as an extraneous bee in the author's bonnet, the foregoing grotesque paragraph contains the intellectual germ which is expanded into the volume of Mr Genet. The "force of levity" becomes in his skilful hands, a sort of man-of-all-work, that carries canal boats from level to level without water, raises stranded vessels, recovers foul anchors, saves drowning men, navigates seas, rivers, and lakes, and at length disappears in a flying machine that would have delighted the heart of Bishop Wilkins himself. The manner of performing all this is put down in letter-press and illustrated by engravings. Take an example. "The moving force," to carry a canal boat to a higher level, "of this machinery is to be essentially the ascensive force of an aerostat [balloon] of a spheroidal form which, when disengaged and allowed to ascend, will exercise its power on the large wheel by the means of a rope fastened to its appendice [bottom] that will pass through a sheeve at the bottom of the pit, to a groove on the rim of the large wheel and embrace its circumference. The large wheel will besides be supplied with handspikes, to accelerate or retard its motion, or use it separately; and also with a pall, to prevent the load from descending whenever requisite, as it will hereafter be explained. The large wheel is to be 30 feet diameter and 90 circumference, [in the time of *Metius* every thing of 30 feet diameter had a circumference of something more than 94 feet; but, *nous avons changè tout cela*,] which proportions will give fifteen revolutions of the drum while the large wheel will perform only one." p. 25. If this is not intelligible the reader will understand it when we inform him that the apparatus consists of wheel work moved by the ascent of a balloon, which, after having risen forty feet is, we are given to understand, "to be recalled down" by "the individual force of the large wheel," and made to perform a new ascent. But why not use the *descendent* force of a leaden weight, as well as the ascensive force of an air-balloon, to "be recalled up," in the same way that the other is recalled down? or better yet, why not carry up the boat by the "individual" exertion of a horse or a steam-engine, as one Robert Fulton proposed a great while ago?

Another specification of the application of the "upward forces of fluids," is, to the navigation of seas, lakes, and rivers. Here Mr Genet's apparatus assumes a somewhat different form. The balloon or aerostat, which before floated in air above, has now become a hydrostat and works in "the floods below." As this new application is founded on an experiment, it may be worth our while to relate it. Mr Genet took "a small hollow balloon shaped like an oblong spheroid, made of tin, containing in its cavity ½ cubic foot." "Several experiments have been made with this simple *hydroaerostatometre*," which prove, "That the uplifting force of salt-water exercised against the balloon filled with nothing but atmospheric air, supported 47 pounds; which, with the three pound weight of the balloon and its appendages, the weight of the cup and friction of the pullies, amounting altogether to 5 pounds, gives to the upward pressure of salt-water against the inferior surface of ½ cubic foot of atmospheric air so contained, an uplifting force of 54 pounds, and to a cubic foot 108 pounds." pp. 36, 37. This is very important, as a foot of salt water weighs about 64 pounds, and it has been heretofore supposed, that no substance, although devoid of weight, plunged under water, could tend to rise with a force greater than the weight of an equal volume of water; but "*nous avons changè tout cela*," again. The apparatus which Mr Genet has contrived, is, as we take it, a *perpetual motion*, and the learned person at the head of the Patent-office ought, from the rules which he prescribed to himself, some years since, to have refused a patent for it until Mr Genet *made it go*.

But the best part of the "Memorial," is the description of the flying apparatus, carrying up 2 horses to *move its wings*, and fly

> "With restless violence,
> Round about the pendant world."

The author says, "In order to form this new plan, I have consulted nature, which offers in every thing the best and most perfect models; and I have ascertained by numerous observations, that the plainest way to perfect aeronautics, was to imitate, as nearly as possible, the innate faculty with which nature has endowed the birds and the fishes, to enable them to raise, propel, and guide themselves in the fluids in which they are immersed." p. 68.

Now his imitation of nature is in the form of a monstrous balloon, which is to be loaded with a wind-mill and two horses, three men, their attendants, a chemical apparatus, an anchor, fodder and provender, water, provisions for the men, and errors and omissions to the amount of 13,400 pounds. This is an imitation of the innate faculty with which nature has endowed birds and fishes, with a vengeance.

Our readers are already sufficiently initiated in the mysteries of the "upward forces of fluids," from the specimens here presented. Few of them we apprehend will follow our unprofitable example, for we have honestly read the *Memorial* from beginning to end.

Art. XL.—*On the Mirage.* Communicated for this Journal.

[Concluded from page 165.]

The appearances, of which an account has been given in the former part of this article, were observed, without any attempt, in most cases, to reduce them to accuracy by the measurement of distances and angles. Wollaston on the Thames, in England, Woltman on the Elbe, Legentil in the Indies and in Normandy, and Humboldt at Cumana, in America, made observations of a more exact nature, in which were noted the temperature of the air and water at the time of observation, the angle of depression, and in some instances the distance of the objects observed. Of these it is unnecessary to give a minute account as no new facts were discovered, and the appearances in general were not very striking.

But the most full and minute account which has been given, and the most important for the mathematical theory with which it is accompanied, and by which the facts are explained, is that of Biot, published in the Memoirs of the Institute for 1809.

He introduces his own observations by noticing the facts and explanations already given. That of Huddart is considered insufficient, as the curvature which he supposes is not necessarily produced by difference of density, without difference of refracting power; and the introduction of aqueous vapour into the air would be as likely to increase the refracting power near the surface of the water as to diminish it.

The explanation given by Dr Vince is considered rather as a statement of the possibility of the formation of the images, than an explanation of the true cause of the phenomenon. A thousand other suppositions would show the possibility of these images as well as this; and as to the supposition that the curves are all entirely concave towards the earth, he believes it to be contrary to fact.

The explanation given by Monge is considered on the whole very correct; and very remarkable, if we recollect that it was suggested by the view of the phenomenon itself, in a camp and amidst the dangers of war.

Other observations are also examined, particularly those of Wollaston, of which Biot justly speaks in the highest terms.

The occasion which led Biot to the observations he records, and which serve as the basis and confirmation of the conclusions to which he is led in his mathematical theory, are thus described by himself.

"The opportunity occurred during the residence of M. Mathieu and myself at Dunkirk, in the winter of 1808, for the purpose of verifying the latitude at the northern extremity of the meridian line. We had resolved to take advantage of the vicinity of the sea to make frequent observations with a repeating circle of the depression of the horizon, by placing ourselves successively at different known elevations, which would enable us to determine the form of the trajectory described by the rays of light, according to the different modifications of temperature of the air and water. During these observations, we discovered a place on the beach where the *suspension*[*] and *mirage* were sensible every day. The visual ray, after having for a great distance passed along this sandy surface, terminated in a multitude of objects, such as steeples, houses, hills, trees, &c. which, appearing suspended in the air and baseless, presented their inverted images beneath them. These images, situated also in the air and above the apparent horizon, had a perfect resemblance to images reflected in water; and the illusion was so complete that we could not avoid being deceived by it, although it was perfectly easy to convince ourselves of our error, as the brink of the apparent water was sometimes not 200 metres distant from us. We often amused ourselves with sending off in that direction men provided with poles, which served as signals. As they receded, they seemed gradually to plunge into this apparent sea, and at last, with their reflected image, to sink beneath its surface. Nothing can equal in appearance the variety and strangeness of these phenomena. They vary with the elevation and distance of the objects, with the nature, and especially the form of the intervening ground, and with the elevation of the observer; your least movement, modifies every appearance; the slightest change of temperature alters them, and a sunbeam is sufficient to destroy or reproduce the whole illusion." pp. 11, 12.

Sometimes, by placing the eye at the distance of about two feet from the surface, distant spires and trees could be seen suspended, as it were, in the air, and the clear bright sky beneath them; on raising the eye, the appearance of sky vanished or became very narrow. Sand hills and huts were also suspended, and some times doubled; and in a few cases, even a third image was seen below the other two. This last image, however, and even the second were sometimes so contracted as to be invisible. In every case of suspension, Biot shows that there is always a second image, but that it is so extremely flattened as not to be seen.

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[*] The case of the direct image of an object being apparent above its true place, and without any visible inverted image is called *suspension*.

The sea-birds, which at first exhibited only a simple and direct image, often presented a second inverted image, and sometimes, immediately after, disappeared. They also sometimes disappeared without presenting a second image. It must have been " a curious spectacle, to see clouds of birds as they rapidly approached the horizon, suddenly double their number and motions," and as suddenly disappear.

In this situation they took the greatest pains to estimate every thing by measurement. The form and nature of the ground, the distance above the surface, and from the observer, at which the different images appeared, the temperature at the time, at different heights from the ground, the depression of the horizon and of objects; the form of the path of the luminous ray and of the limiting line, were all ascertained by careful and numerous experiments. It was evident that the frequency and distinctness of the phenomena were owing to the particular form of the sandy surface produced by the reflux of the tide on the beach.

In order to arrive at the true theory of the phenomena, Biot traces the motion of light in media, whose different strata have different refracting powers. Its path or *trajectory* is first ascertained in a particular case, real or hypothetical, and thence deductions are made for the general theory. The trajectories are considered as passing from the eye to their lowest point, or point of reflection, and this portion of the path is called the first branch, the portion beyond the point of reflection being called the second branch. The line produced by uniting the points of reflection of rays reaching the eye from different points is called the *limiting curve* or *caustic.* Now it is shown that any object, not too near the observer, situated between this caustic, and the highest of those trajectories which have their lowest point below the level of the eye, will send to the eye two images, one formed by the direct rays, which will be erect, and the other formed by rays reflected at some point of the *caustic*, between and below the eye and the object, and which will be inverted. Rays from all objects below the caustic will pass below the eye, and such objects will be invisible. If the lower part of the object were situated on the limiting curve itself, the interval between the two images would be reduced to nothing, and they would seem to touch each other.

What has here been said belongs particularly to the case in which the stratum of air in contact with the earth has the least refracting power, and the refracting power increases in the successive horizontal strata at different distances from the earth, in arithmetical progression. By whatever law, however, the refracting power increases, it is shown that analogous results are deduced. Wherever the refracting power increases upward, there exists below a certain trajectory, which is the lowest of those which are refracted upwards and reach the eye, and which is thence called the *trajectory limit*, a space entirely invisible.

All these results, which are rigorously deduced from theory are entirely confirmed by the observation made at Dunkirk.

While the man, furnished as already observed with a pole, on which were marked metres and decimetres, was between the observer and the lowest point, or point of contact of the trajectory limit, his image was seen single, direct, and entire. At this point he seemed to enter the water; as he receded, the lower part of his body disappeared, as if plunged beneath the surface. At a greater distance, the image of a portion of the body seemed to be reflected from the surface of the water; and just before he seemed to disappear beneath its surface, the upper part of the body was fully and clearly reflected: thus giving a double image between the lines called the trajectory limit and caustic curve, and below the latter becoming entirely invisible.

If the successive strata vary in refracting power to a certain distance and beyond this are invariable, and the observer be placed at the limit between them, an object may be so placed with regard to him as to send four images alternately erect and inverted. If the refracting power diminishes in the successive strata in arithmetical progression, two erect and one inverted image placed between them, may always be seen of objects placed in a suitable position.

To these cases may be referred most of the remarkable appearances mentioned by Vince and Scoresby.

If the refracting power of the medium increases upwards to a certain distance, and afterwards diminishes indefinitely, and the observer be placed at the point of greatest density, or a little above or below it, a very great number of images of an object situated in certain positions may be seen at the same time. This curious result of the theory is confirmed by phenomena observed by Biot and Arago in Spain.* They were stationed on the mountain Desierto de las Palmas in Valentia, at an elevation of 727 metres above the sea; and were observing by night, with a repeating circle, the signal lights fixed on the mountain of Campvey on the isle of Yviza, at the elevation of 420 metres, and at the distance of 41½ leagues. This distance was one of the sides of their great triangle. The light at first appeared single, like a very small star, as usual; but presently after, in the course of their observations, they noticed, directly above the first, another light, which they took to be a star. As this supposed star did not change its place, they naturally concluded that it was a second image of the light, and continuing to observe, they saw two, three, four, and even more lights. These successively appeared and disappeared in an irregular manner, and they observed the highest lights seemed largest and most brilliant. Somewhat analogous to this was the occasional appearances seen at some other stations, of a single luminous point, which was sometimes observed, to lengthen itself vertically into a luminous column. This column he considers as made up of an infinite multitude of contiguous images.

Biot next examined the apparent depression of the horizon of the sea, as observed from stations elevated at different heights above the surface of the sea, noting always the difference of temperature of the air and water, which he shows to be the important circumstance on which the difference of depression at different times depends.

From these he infers, that "rays of light which enter the atmosphere at small inclinations, experience, as they approach the surface of the earth, considerable disturbances, which are produced by the influence of this surface on the temperature of the inferiour strata of the air. This influence usually becomes insensible at a small elevation, above which the density of the air gradually decreases in arithmetical progression. This gives the rays a curvature directed towards the surface of the earth. But, in the lower part of its course, the trajectory may be more or less concave or convex, according to the temperature of the surface, it may thus experience in its course several successive inflections, and, contrary to the idea generally acted on by astronomers, it will rarely happen that the curvature of the superiour part is applicable to the smaller elevation.

"Thence result several useful consequences. The first is of importance to navigators. When they take altitudes, making use of the apparent horizon of the sea, they must often experience the effects of extraordinary refraction, and, however little the sea may be colder or warmer than the air, be exposed to errors of 3 or 4 minutes in the latitude from this cause alone. For the same reason, the latitude observed on board a canoe may differ very sensibly from the latitude observed from the deck of a lofty ship. This happened several times at New Holland, in the expedition of Captain Baudin, without any assignable reason for the difference, and notwithstanding all the care that was taken to avoid it. Wollaston proposed to remedy the inconvenience by observing th distance of the two opposite horizons, as the difference between this observed distance and 180° might be attributed to refraction; but it is not always possible to make this observation, especially near coasts and in situations where the necessity of having the latitude exactly is greatest. In every case it is only necessary to observe the temperature of the air and of the sea, to know in what direction the error is to be allowed for.

On land we may always avoid the error by observing with an artificial horizon placed at the distance of 3 or 4 metres from the ground; for the effect of the extraordinary decrease of density being much diminished and generally insensible at this height, the visual ray is only subjected to the ordinary law of refraction.

"The same cause may affect the elevation of stations, when calculated from the apparent depression of the horizon of the sea; for, if the sea be warmer than the air, in which case the lower part of the trajectory will be concave towards the surface of the earth, the observed depression will be greater than in the ordinary state of the atmosphere. The elevation calculated according to the ordinary law of mean refraction will consequently be too great, and if the sea be colder than the air, it will be, on the contrary, too small."

These positions are proved by calculating the elevation of several stations used in the observations at Dunkirk, first from depressions taken when the water was warmer than the air, and afterwards, from those taken when the air was warmer than the water. In the former case the calculated elevations were much too great, and in the latter much too small. The difference between the calculated and measured elevations was least in those stations which were most elevated.

The obvious conclusion from these facts, which seem to be ascertained with such extreme accuracy, that entire dependence may be placed on them, is, that trigonometrical observations, made over the water, are liable to causes of inaccuracy, which cannot be always easily estimated, and to guard against which, in some degree, the temperature of the air and water at the time of observation should always be taken, and allowed for.

General Intelligence.

Improved Cement for holding small Lenses, whilst grinding and polishing them.—In grinding small lenses, Mr Pritchard found that shell-lac, the cement usually employed for them, was by no means sufficiently strong to retain them. He was fortunate enough, however, to attain his object by adding to the shell-lac an equal weight of finely levigated pumice, carefully melting them together in an iron vessel, and stirring them till well incorporated. Great care is required in using it, not to heat it hotter than is absolutely required in melting it, and in fixing the lens securely, otherwise it becomes unfit for use; and the same caution is equally required in using shell-lac alone.—*Edin. Jour.*

Remarkable Waterspout.—This extraordinary phenomenon was seen on the 6th July 1822, in the plain of Arsonval, a village about six leagues W. S. W. of St. Omer, and six from Boulogne. About 1^{h} 35′ P. M. clouds coming from different parts collected rapidly, and formed a single one which covered the whole horizon. From these clouds a thick vapour immediately descended in the shape of an inverted cone, and having the bluish colour of burning sulphur. The apex of the cone next the earth revolved with great velocity; and formed an oblong mass of about *thirty* feet detached from the cloud. It rose with a noise like the bursting of a large bomb, leaving a hollow in the ground, of from 20 to 25 feet circumference, and from 3 to 4 feet deep in the middle. It then set off in the direction of W. to E. knocked down a barn, and gave to the neighbouring farm-house a shock like that of an earthquake. It overthrew from 25 to 30 trees, and laid them in such a variety of directions, as to prove that it advanced with a rotary motion. It next travelled through a distance of two leagues without touching the ground, carrying off huge branches of trees which it threw out with great noise to the right and left. Having reached the highest point of the wood Fauquembergue, it carried off the tops of several oaks, which it took along with it, below the village of Vendome, at the foot of the hill on the east of the forest. In that commune it did no other harm than to carry off a very large sycamore tree, root and branch, to a distance of 600 paces.

Continuing its route like a ball fired *en ricochet*, it attacked the village of Audinctun, where it demolished the roofs of three houses, and carried away several trees, among which were five ashes of great height. After quitting the valley, at the mouth of which these two villages are situated, it ascended the mountain de Capelle, and several ploughmen saved themselves by lying down and holding firmly by their ploughs, one of which was driven so deep into the ground, that three horses were unable to pull it out. M. Demarquoy, who has described this phenomenon minutely, observes, that its form (the form of its section, we presume,) was oval, its length being about 30 feet, and its other diameter about 20. The spout turned, in its progress, each of its faces to all points of the horizon. Globes of fire issued from its centre, and often globes of vapours like sulphureous ones. Its noise was like that of a loaded wagon dragged at a gallop over a paved road. Every globe of fire or vapour that issued from it, was accompanied with an explosion like that of a musket, and the wind, which was violent, added to this a terrible whistling noise. Near Mont Capelle, it penetrated into the vallies of Witernestre and Lambre. The first contains about 40 houses, 32 of which, with their barns, were overturned, and an enormous quantity of trees beat down, torn up and carried to a great distance. At Lambre, the revolving motion of the meteor was seen, and also its sulphur brown colour, and from its centre, like that of a fire, there issued flashes of bituminous vapour. The trees round the church were broken and uprooted. The house of the curate was unroofed, and eighteen houses, most of them built of brick, were sapped to their foundations, and exhibited the extraordinary phenomenon of the separation of the walls, which were thrown outwards. After quitting Lambre, the spout divided itself; one part was dissipated, but the other went to Lillers, about three leagues from Lambre, where it broke and uprooted nearly 200 trees on the fine meadows of M. Defoulers, before it vanished. At three o'clock the weather became calm, the sky serene, and the thunder terminated with the waterspout. *Bull. des Sc. Phys.* Avril, 1824, p. 236—239.

Highly calcined Charcoal a Conductor of Caloric.—M. Cheuvreusse has found, that charcoal when highly calcined is a perfect conductor of caloric. When the charcoal is not much calcined and is dry, it does not conduct caloric. M. Cheuvreusse proposes to use the first of these charcoals in place of copper in galvanic piles, and also for the purpose of carrying electricity into the ground from conductors.—*Ann. de Chim.* Tom. xxix. p. 440.

New Experiments on Flame.—It appears from a series of experiments by Mr Davies of Manchester, that there is considerable foundation for the opinion of Mr Sym, that the flame of a candle is a conical surface, the interior of which is not luminous, a section of the flame being a narrow luminous ring surrounding an obscure disc. Mr Davies found that there is no oxygen in the interior space within the red flame, as phosphorus and sulphur, though they readily melted, yet never would inflame when placed within it. These inflammable bodies, however, always burned when the flame was blown upon them with a blowpipe, so as to supply them with oxygen. The moment the supply of oxygen was exhausted, they were again extinguished. Mr Davies explains the great power of the oxy-hydrogen blowpipe, by stating, that in it the flame, instead of being a superficial film of inflammation, is a solid mass of fire.—*Ann. of Phil.* Dec. 1825, p. 447.

Temperature of different Animals.—Mr Despretz has obtained the following results on the temperature of different animals, that of the air being 59° Fahrenheit.

Two Carps, temperature of water 51·4°	53·0° Fahr.
An adult Guinea Pig	96·4
3 male Children, aged between 1 and 2 years,	95·2
4 young Persons aged 18	98·6
9 Men aged 30	98·85
4 Men aged 68	98·83
A Dog three months old	103·06
Three Pigeons	109·37

Respecting the cause of animal heat, M. Despretz has drawn the following conclusions:

1. That respiration is the principal cause of animal heat, producing seven-tenths of it in very young animals, and often as much as nineteen-twentieths of the whole effect; the re-

maining heat being produced by assimilation, the motion of the blood, and the friction of the different parts.

2. That besides the oxygen employed in the formation of the carbonic acid, another portion of gas, sometimes very considerable in relation to the first, disappears also; more oxygen disappearing in general in young than in adult animals.

3. That there is an exhalation of azote in the respiration of mammiferous, carnivorous, and granivorous animals, and in the respiration of birds; and that the quantity of azote exhaled is greater in granivorous than in carnivorous animals.—*Bull. des Sc. Nat.* &c. Avril, 1825, p. 244—246.

Experiments on the Compression of Air and of Gases.—These experiments were made by M. Oersted, with the assistance of M. Suenson. The most powerful compressions were made in the breech of an air-gun, in which they succeeded in compressing air to 110th part of its original volume. It was found that Mariotte's law was preserved in these high pressures. In their next experiments, which were made on gases, they succeeded in establishing the existence of the same law, even when these gases were about to be converted into liquids. M. Oersted remarks, that, in liquids, the compressions equally follow the proportion of the compressing force, and that it is extremely probable that solids are subject to the same law. He therefore concludes, that this simple law, That the diminution of volume is proportional to the compressing force, holds in each of the three classes of bodies. He adds, that this law can only be admitted on the supposition that the caloric developed by compression has been permitted to escape before the measurement is made.—*Dublin Philosophical Journal.*

Magnetizing Power of Light.—Mrs Mary Somerville, one of the most highly gifted and accomplished females of our time, has lately communicated to the Royal Society of London a memoir on the magnetizing power of the more refrangible rays of light. From the beautiful experiments detailed in the communication, Mrs Somerville infers, *that the more refrangible rays of light have the property of imparting magnetism.*

Notice regarding Steatite or Soap-stone, and its principal Uses.—Steatite is, as is well known, a variety of the talc genus. Its colour is white, green, or grey; it is also sometimes, though rarely, red and yellow. Its specific gravity varies from 2,60 to 2,66. It is a compound of silica, alumina, magnesia, oxide of iron and water, which vary according to the locality. It is very common in Cornwall and Germany. As it is fusible only at an exceedingly high temperature, and is easily wrought, excellent crucibles may be made of it, which are further hardened by fire, and which are only with great difficulty penetrated by litharge. It is also employed in making moulds for melting metals. In England it is used in the manufacture of porcelain. M. Vilcot, an artist of Liege, made several trials of it with the view of finding out whether it might not be susceptible of being employed by the lapidaries. He prepared cameos of this substance, the colour of which he brightened in the fire, and which he rendered so hard by the elevation of the temperature, as to give sparks with steel. They were then coloured, yellow, grey, or milk-white, by different solutions. He polished them upon the stone, and ended with making them assume all the lustre of agate. Some pieces even resembled onyx in colour; but a serious inconvenience was, that the markings were easily altered by the fire, and could no longer be restored. Steatite has a great affinity for glass; it is also employed, in the manner of paste, reduced to a fine powder, and mixed with colouring matters, for painting upon this substance. It also serves as a sympathetic crayon for writing upon glass; the traces seem effaced, when a piece of woollen cloth is passed over them, but they re-appear immediately when moistened by the breath, and again disappear when the glass becomes dry. Steatite is not so easily effaced as chalk, and does not, like that substance, change its colours. Tailors and embroiderers also prefer it to chalk, for marking silk. It possesses the property of uniting with oils and fat bodies, and enters into the composition of the greater number of the balls which are employed for cleaning silks and woollen cloths; it also forms the basis of some preparations of paint. It is employed also for giving lustre to marble, serpentine, and gypseous stones. Mixed with oil, it is used to polish mirrors of metal and crystal. When leather, recently prepared, is sprinkled with steatite, to give it colour, and afterwards, when the whole is dry, it is rubbed several times with a piece of horn, the leather assumes a very beautiful polish. Steatite is also used in the preparation of glazed paper; it is reduced to very fine powder, and spread out upon the paper; or it is better to mix it previously with the colouring matter. The glaze is then given to the paper with a hard brush. It facilitates the action of screws, and from its unctuosity, may be employed with much advantage, for diminishing the friction of the parts of machines which are made of metal.—*Jameson's Edin. Philo. Jour.*

Obituary.—SAMUEL PARKES, Esq.—This gentleman, so well known by his publications on chemistry, died on the 23d of December. "His works attest his ardour, diligence, and perseverence in the pursuit of science; nor was he less distinguished by his beneficent efforts and pecuniary liberality in the support of every public institution which tended to increase the happiness or promote the improvement of his fellow creatures. His industry and activity of mind were evinced even during his last illness, by his being anxiously engaged in preparing and superintending improved editions of his chemical works."—*Philos. Mag.*

Celestial Phenomena for the months of June, July, and August, 1826, *computed for Cambridge, in lat.* 42° 28′ 28″ *N. and long.* 71° 7′ 25″ *W.* Mean time. Communicated by R. T. Paine, Esq.

June, 1826.

D.	H.	M.	S.		
4	9	27	18	P. M.	Im. 4th Sat. ♃.
"	9	28	33	"	Em. 3d Sat. ♃.
5	1	07	56	"	☌ ☉ and ☽, ☉ eclipsed, invisible.
10	5	05	10	"	✱ ☌ ☽ and ω ♌.
11	10	15	29	"	Im. 3d Sat. ♃.
13					♂ stationary.
14	10	03	21	"	Em. 1st Sat. ♃.
16	10	48	42	"	Em. 2d Sat. ♃.
17	3	01		A. M.	☌ ☉ and ♄.
19	10	43	24	P. M.	☌ ☽ and 1 μ ♐.
20	11	51	16	"	☌ ☽ and *d* ♐, a near approach.
21	8	01	09	"	☉ enters ♋, summer begins.
23	0	0	44	A. M.	☌ ☽ and ν ♒.
24	0	10		P. M.	Sup. ☌ of ☿.
30	4	0	13	A. M.	☌ ☽ and π ♈.

July, 1826.

D.	H.	M.	S.		
3	11	38	51	A. M.	✱ ☌ ☽ and ζ ♉.
7	10	38	34	P. M.	☌ ☽ and ω ♌.

D.	H.	M.	S.	
13	8	0	31	P. M. ☌ ☽ and ♑.
15	0	20		A, M. ☍ of ♅.
22	3	0	10	“ ☌ ☽ and Situla, a near approach.
23	6	56	49	“ ☉ enters ♌
“	8	33	56	P. M. Em. 1st Sat. ♃.
29	7	18		“ ☌ ♀ χ ♌ ♀ 9′ s. of ✱.
31	6	20		A. M. ☌ ♄ and η ♊, a near approach. ♄ 2′½ s. of ✱.

August, 1826.

D.	H.	M.	S.	
1				Great. Elong. of ☿, East of ☉.
“	4	0	28	A. M. ☌ ♀ and ♃, a very near ap. ♃ 1¾′ s. of ♀.
8	6	37	31	P. M. ☌ ☽ and ι ♍.
9	1			“ ☌ ♀ and β ♍ ♀, 3′ N. of ✱.
10	9	20		“ ☌ ☽ and ♑, at nearest ap. ☽’s N. limb about 20′ apparently S. of ♂.
11	11	04	05	“ ☌ ☽. and θ Ophiuchi.
16				☿ Stationary.
17	2	20		“ ☌ ♄ and μ ♊, ♄ 7′ S. of ✱.
22	1			“ □ of ♂, East of ☉.
23	1	20	14	“ ☉ enters ♍.
24	1	15		“ ☌ ♂ and δ ♏, ♂ 16′ S. of ✱.
27	3	46	07	A. M. ☌ ☽ and ζ ♉, a near approach.
30	9	30		“ Inf. ☌ of ♉

In the eclipse of the sun of the 5th of June, about two-thirds of his disc will be obscured near the Antartic circle and long. 90°, but the eclipse will no where be central, as the parallax of the moon is less than her latitude. It is perhaps worthy of remark, that in these three months, the moon will not at Cambridge, occult any star, whose magnitude is not less than the fifth.

Boston, May 22, 1826.

Art. XLI.—*A Review of some leading Points in the Official Character and Proceedings of the late President of the Royal Society.* By a Correspondent.—[*Phil. Mag.*]

[Concluded from page 319.]

I will now quote again from the "History of the Instances of Exclusion;" its author is speaking of

"The formation of every Council since Sir Joseph's presidency, but particularly of the last Council. The Council of the Royal Society is, at the same time, as is well known, its committee of papers, that part of the body who is to decide upon the merit of discoveries, either foreign or domestic, and to hold the equal balance between its own laborious and ingenious members. The nomination of this body is in the Society at large, who, however, in a very evil hour, have of late in fact left it to their President, evidently under an implied, though not expressed covenant, that he would take care there should always be in it a proper number of men of science in each branch qualified to do the work for which they are deputed. * * * But what lists are there put into the balloting-boxes this year? * * * Where are the mechanics? Where are the professed chemists? Where are the mathematicians? Where are the practical astronomers? What! not a single astronomer in the Council of the Royal Society of London, instituted for the promotion of natural knowledge, at a time when the heavens, almost shut up since the creation, have been unfolded by Herschel to the curiosity of mankind? Is it possible? and do we affect (for affect it we must) to be seriously uneasy, because we suspect that some foreigners may not have had answers in form to their letters of form, while this is our shame and this is our disgrace? * * * * What then is to become of the papers, and by whom are they to be tried? Formerly there was some kind of established order in the Society; the learned man gave his papers to the Secretary, the Secretary in due time produced them to a body of men that was known, and each of whom was responsible for the sentence he gave. The *President* now takes them, the President changes the order of reading them (not in particular cases, as alone he is permitted by exception in the statutes, but every Thursday); the President may if he pleases hand them about to a junto (he must hand them about to somebody), for an opinion, who may be the enemies or the rivals of the writer. Whoever sees not in this as well as in the imperial ludi-magisterial knock with the hammer, in the dictatorial rebuff; in the nomination of Dr Dryander to take the catalogue of the books when there was a Librarian in the house, and in the attempt to *dismiss* the said clerk and librarian unheard and unadmonished,—sees not a fixed and settled plan of despotism, not less violent in the means than trifling in the object, must have been born blind, or have made himself so."

"The President is incurably sick with the lust of domination;* he imagines himself born to rule (Good God! how little do men know themselves!); and cannot perceive that he has neither the intellectual nor the moral qualities of a ruler. Honesty he possesses—the honesty of a private man. Of the honesty of a governor, for which modern languages have no name, the Greeks called it ἐπιείκεια, he is destitute."

"We have not written thus far to dissemble what we think. Sir Joseph Banks *might* make a very good clerk, a very good attorney, or even a very good treasurer to the Society; but the man who is to fill the place of President should be something more. We hear much of his hospitality, and of his public breakfasts; but surely the papers of Messrs. Cavendish, Kirwan, and Herschel; of Dr Maskelyne, Dr Hutton, and Mr Wales; of Mr Vince, Dr Waring, Mr Hunter and Mr Maseres, (and take these away, what so great remains?) would have been produced, though there had been no breakfasts in Soho Square; and *who knows* after all (we speak upon *more* than conjecture) how many papers have been stifled, and how many *subjects* of science have been discouraged, by the same caprice and love of dominion, which has dictated so many other innovations? * * * * The Royal Society *was* a *Society;* we do not wish to see it a *monarchy*: it *did* conduct itself according to the rules of justice and equity; we desire that it may not violate those rules; its principles *were*, that the first distinction of men is virtue, and the second learning; we cannot bear that birth (merely *as* such) should take rank with either of these. Now, the President does think that it ought (and forces his noble friends upon us accordingly), and *therefore* it is proper to look out for one who with Sir Joseph Banks's merits, be those merits what they may, does not think so."

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* As an evidence of the manner in which the President dictated to the fellows in the choice of officers, I here insert a verbatim copy of an engraved card circulated to recommend Dr Blagden.

"In consequence of Mr Maty's resignation of the Secretaryship at the last meeting of the Royal Society, the President takes this method of acquainting you, that, *at his desire*, Dr Blagden has declared himself a candidate for that office. From Dr Blagden's known abilities and habits of diligence, the President does not doubt but he will, if elected, fulfil the duties of the station with advantage to the Society.

"Soho Square, March 29, 1784."

The card, of which the above is a copy, is now in my possession.

Such, according to the representation of those who were active members of the Royal Society, nearly forty years ago, were the causes of the dissensions by which it was then agitated. In the course of these disputes Sir Joseph and his friends formed the plan of removing Dr HUTTON, then Professor of Mathematics in the Royal Military Academy at Woolwich, from the office of Foreign Secretary, which he had discharged with great honour to himself, and perfectly to the satisfaction of a majority of the members.

Here, again, that I may not, however involuntarily, slide into any discolouration of circumstances, I shall quote a pamphlet published expressly on the subject, and entitled, "An Appeal to the Fellows of the Royal Society, concerning the Measures taken by Sir Joseph Banks, their President, to compel Dr Hutton to resign, &c."

"Dr Hutton is known to be one of the best mathematicians in England: and he is likewise a very good writer upon the mathematics, which is far from being the case with every person who understands them, or is well read in them. He is also remarkably industrious, and has furnished the Philosophical Transactions with more papers (and those full of ingenuity as well as learning), upon mathematical subjects, than, I believe, any other member of the Society. He is also Professor of Mathematics at the Royal Military Academy at Woolwich, where he some years ago tried a variety of most curious and useful experiments upon the force of fired gunpowder, and the initial velocity of cannon-balls, similar to those which had formerly been tried upon the like subjects with respect to musket-balls, by the late very eminent mathematician and engineer, Mr Benjamin Robins. And he afterwards drew up an account of these experiments upon cannon-balls, which was presented to the Royal Society, and printed in the Philosophical Transactions; the Society rewarding its learned author by giving him Sir Godfrey Copley's medal."

It was on account of Dr Hutton's eminent abilities and his extraordinary activity* in devoting them not to purposes of ceedings in the Royal Society, to those in which he engaged with Dr Priestley. But Hutton and Maskelyne continued with unabated ardour to promote science and philosophy in the departments which they had respectively chosen; Hutton as an active conductor of experiments and a most sedulous and successful author on mathematical subjects, in all their variety, carrying on at the same time a more extensive correspondence with mathematicians at home and abroad than any other man in England; Maskelyne as an unwearied observer of the heavens, and as the superintendent and director of the *Nautical Almanac*, an important work, to which he gave a correctness altogether unequalled in any similar publication, or even in that, since it has fallen into other hands.

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* Such is the ignorance of the writer in the *New Times*, that he speaks of *Simpson*, *Hutton*, &c. as mere elementary writers for school-boys. Were he

Although, however, these distinguished individuals quitted the Society for the sake of *peace*, they were not permitted to remain in peace. The President continued for years to annoy them with a petty but inextinguishable malignity. His opposition to Dr Maskelyne evinced itself most frequently in the *Board of Longitude*, of which he was, *ex officio*, a member. Sir Joseph was altogether ignorant of nautical astronomy, of the construction of nautical instruments, and the principles of mechanics employed in the construction of new apparatus; but notwithstanding these disqualifications, he was incessantly setting up his judgment against that of the Astronomer Royal. If the Astronomer Royal brought forward any new plan of nautical improvement, the President of the Royal Society was sure to oppose it. If the Astronomer Royal recommended a new instrument, the President's opposition followed as naturally as night succeeds day. This habitual opposition by degrees brought the Oxford and Cambridge Professors (who were also *ex officio* members of the Board) to be uniform supporters of Dr Maskelyne; and this soon led Sir Joseph to regard them as *his* enemies. Determined to maintain his ascendancy, notwithstanding the opposition of this powerful phalanx of men of science, he laid a plan for remodelling the Board. This he could not carry during Dr Maskelyne's lifetime; but by dint of perseverance and intrigue he at length succeeded, and in 1818 had influence enough to effect such an entire change in the constitution of the Board, as brings it under the management of a little committee in London, and, whenever it is necessary, throws the Oxford and Cambridge professors, certainly *the most useful members of the Board*, into a complete and decided minority. Thus, to adopt the language of the *New Times*' eulogist, has "the Presidency been *in honour and activity*."

Often, again, has the dislike of Sir Joseph to this eminent astronomer been manifested by causing to be "black-balled" candidates whose certificates Dr Maskelyne had signed. From twenty instances of this kind which I could specify, I shall only select one. Mr *Stephen Groombridge*, residing at Blackheath, in the immediate vicinity of the Royal Observatory, maintained a friendly intercourse with Dr Maskelyne, and became himself a most sedulous astronomical observer, having at considerable expense provided excellent instruments for that purpose. About two years before Dr Maskelyne's death, he (with others) signed the usual certificate recommending Mr Groombridge as a fit person to be a fellow of the Society. The signature of Dr Maskelyne was fatal. But Sir Joseph put the rejection of Mr Groombridge upon another pretext. Mr Groombridge, it seems, has a mercantile occupation in London. Sir Joseph, therefore, actively directed his appropriate observations to the danglers at his levees during the twelve weeks' probation. "*O! ho! we are to have London tradesmen thrust upon us, are we? I am astonished at Dr Maskelyne. But we will not degrade the Royal Society by the admission of London tradesmen!*" A fine illustration, truly, of the avowed principles of the Society in 1693, quoted at the beginning of this review.*

The sequel of this affair should not be omitted. In two or three years afterwards Mr Groombridge was much extolled by French and German astronomers on account of the accuracy and utility of his researches. A new attempt was made

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* It would be easy to fill a volume with miscellaneous examples of the capricious exercise of the President's power with regard to exclusion. A single example shall be placed in this note. A distinguished physiologist was proposed as a candidate; his certificate being signed by two noblemen, by a member whom, to avoid circumlocution, I will call Mr A. C., and by three other members. On the evening of election Mr A. C. observed Dr *Dryander* (who, though not a fellow, was usually very busy on these occasions) trotting about from fellow to fellow, and whispering to each. Ere long he came to him: "The President's compliments, and he will thank you to *black-ball* this candidate." "Give my compliments to the President (rejoined Mr A. C.), and say, that though I might be happy to oblige him on ordinary occasions, it would not be *decent* now, as my name is on the certificate." By and by, on examining the balloting box, the President exultingly exclaimed, "*All black-balls but one!*" It seems the candidate had no other friend than Mr A. C. present. But what, the reader will inquire, was the reason of his exclusion? Simply this: the names of *two noblemen* were on his certificate! Had only one nobleman signed, it would have added weight to the testimonial; but TWO *noblemen* were usually interpreted to indicate a wish to overpower the President; and then it was, "*We'll show them who's who: no undue aristocratical authority here!*"

(Maskelyne being dead) to get him elected; and the Royal Society was actually "*degraded* by the admission of a London tradesman." Poor Sir Joseph! "O consistent spirit of inconsistency, how harmonious are all thy blessed operations!"

On the death of Dr Maskelyne in 1811, (twenty-seven years after the dissensions,) some of his friends informed Mrs Maskelyne that his library, which contained a judicious selection of the best books connected with astronomy in all languages, would be a valuable acquisition to the Doctor's successor, whoever he might be. She therefore offered the whole library to Government on a fair valuation. The members of administration to whom this proposal was made were at first disposed to accede to it; but, on consulting Sir Joseph Banks on the subject, who, as President of the Royal Society, was one of the *visitors* of the Observatory, he depreciated the value of the library, and persuaded them to decline the offer. The consequence was, that the library was sold by auction, and agents employed by Sir Joseph selected during the sale those books which they thought most valuable.

Similar to this, both in kind and in operation, was the hostility manifested by Sir Joseph to *Dr Hutton*. *His* friends, as well as Maskelyne's, if they wished to become fellows of the Royal Society, were regularly excluded; or, sometimes, with the utmost *civility*, informed before hand, that they "had better not expose themselves to the risque and mortification of rejection." If any of them presented papers to the Society, they had the honour of being carefully lodged in the archives of the Society, where the world in general, or even the members of the Society, would derive no more benefit from them than if they were deposited at the centre of the earth. This was the case with memoirs presented by *Wildbore*, *Vince*, *Lax*, *Mudge*, &c.

About the year 1816, Dr Hutton having, by reason of his advanced age, formed a determination to relinquish the habits of a student and the active pursuits of an author, resolved, in consequence, to dispose of his library. He was strongly induced to form this resolution, on being informed by some of his scientific friends, that there were scarcely any mathematical books in the British Museum, and that it was exceedingly probable the governors would be glad to enrich it with so valuable and complete a library as his, if they were properly applied to. The views and wishes of several of the governors of that national establishment were hereupon ascertained; and they were found to be generally favourable to the suggestion of Dr Hutton's friends. The Doctor announced that it was not his wish to make money by the sale of his books, but simply to have them all deposited permanently in some suitable place; and that, therefore, he would most cheerfully abide by the valuation affixed by two persons, one to be appointed by each party. To this proposal many of the governors were well inclined, and they actually appointed one of their officers to take an inventory of the books, and report upon them. The report was favourable. In this state of things, Sir Joseph Banks being then in Lincolnshire, Dr Hutton, fearing he might take offence if not apprized of what was going on, wrote to communicate the requisite information, and to express his hopes that the proposal would be approved by Sir Joseph. To this letter the Doctor received no reply; but in less than a fortnight after he had despatched it, he was informed that Sir Joseph was in London, and busily employed among the other governors of the British Museum in dissuading them from the purchase! From that moment all negotiation ceased. Thus, it seems, does malignity sleep in the breast of a "genuine philosopher," for more than 30 years; when, on a suitable occasion, it starts from its slumber, and proceeds to exert itself with all its primitive virulence.

It would be curious to contrast this proceeding with the grateful eagerness with which Sir Joseph promoted the "job" (as it is universally designated) of transplanting the late Dr Burney's library to the British Museum. But I forbear.

The Trigonometrical Survey of England and Wales was placed by the Duke of Richmond under the direction of Colonel *Williams*, and Lieutenant (afterwards General) *Mudge*, on the recommendation of Dr Hutton. Here was a double cause of offence to Sir Joseph: 1st, in not confiding the superintendence of the Survey to him (for which, however, one of his grooms was just as competent as himself): 2dly, In acting upon the opinion of Dr Hutton, to whom *he* had long evinced an inveterate hostility. The result, alas! too natural in a man of his deposition, was, that for a series of years he continued to oppose the Trigonometrical Survey, and to traduce the character of its conductors. The accounts of the Trigonometrical Survey had been regularly published in the Philosophical Transactions; but at length, through the instrumentality of Sir Joseph, further accounts were excluded. The conductors then laid them before the world in a separate volume; but this was a new occasion of offence. A few years afterwards, a foreigner, *Don Joseph Rodriguez*, was employed to deteriorate the reputation of Colonel Mudge's operations, and to detect, or pretend to detect, mistakes. This foreigner was received by Sir Joseph with open arms, and his memoir was inserted in the Philosophical Transactions; although the paper on which Rodriguez animadverted had not been admitted, and although Sir Joseph *knew* that at the time when he and "his Council" admitted the strictures of this foreigner, Colonel Mudge was too ill to *read* them, much less to reply to them. In that exigency, however, Dr *Gregory*, of the Royal Military Academy, undertook to expose the fallacy, inaccuracy, and illiberality of Don Joseph's animadversions; and such was the success of his efforts, that from that moment the President of the Royal Society was glad to slink out of his opposition to the Survey, and to lay the blame of the encouragement given to Rodriguez upon one or other of the coterie of danglers who then surrounded him. From that period he began to smile upon the man whom he had previously so deeply injured, and Colonel Mudge was found among the visitors at Soho Square! But ingenious men can account for the strangest phenomena; and such have affirmed that Sir Joseph's recent behaviour to the conductor of the Trigonometrical Survey, was only a temporary cloak assumed for awhile, to be laid aside as soon as a particular purpose was accomplished. Sir Joseph had large estates in Lincolnshire, of which he wished to possess an accurate map. None could execute this work so well as the Ordnance Surveyors; and when it was finished, a few guineas would purchase the map of the whole county which included Sir Joseph's estates. How much better than to employ a surveyor at his own expense! A fine project, truly. But, alas! death has defeated it; and ere now both the great man, and the placable individual whom he endeavoured to cajole, have learnt the vanity of every pursuit except those which were consistent with man's ultimate end, and conducted upon principles which will be recognized at the final day of account.

Much should I rejoice if I could, consistently with justice, omit to record any other instance of this lamentable implacability. The task, however, is so repugnant to all the better feelings of one's nature, that a single additional instance is all I shall adduce.

Six or seven years ago, a gentleman named *Marrat*, who had attained a very respectable reputation as a man of literature and science, and who was then, I believe, a bookseller, at Lincoln, undertook a History of the County of Lincoln under the auspices of Sir Joseph Banks. The work was to be published in periodical numbers or parts; Sir Joseph engaging to give it his warmest recommendation, as well as to furnish documents from his private library, in illustration of the history &c. of those portions of the county in which his own estates lay. Relying upon these engagements, Mr Marrat pursued his labours. The work did not obtain a sufficient sale, as it proceeded, to defray its own expenses; but its author, urged by Sir Joseph to persevere, relaxed not. Pleased with the attentions of Sir Joseph, he presented to him a copy of a "Treatise on Mechanics," which he had published in 1810; who, much to the astonishment of Mr Marrat, *immediately withdrew his patronage.* Again and again the historian of Lincoln wrote to Sir Joseph, humbly reminding him of his promises, hinting at the expenses in which he had been involved in consequence of those promises, entreating Sir Joseph to furnish the documents which he had engaged to supply, and without which the work could not proceed, and urgently explaining how ruinous to himself the whole transaction must be, unless, by being enabled to *complete* the publication, he might have some probability of remuneration. But his letters, his expostulations, and his arguments were all in vain. No answer could he obtain; and though he, at length, employed a common friend, who had been present in Sir Joseph's library when the promises were made to Mr Marrat, still nothing could overcome the great man's inflexible silence. What will the reader conjecture was the occasion of this extraordinary behaviour? I blush for human nature, while I tell him, that this unfortunate book of mechanics, in so luckless an hour presented to Sir Joseph Banks, was *dedicated to Dr Hutton!* to the man who, *between thirty and forty years before*, had offended the President of the Royal Society!!

Poor Mr Marrat's circumstanes became so embarrassed by reason of Sir Joseph's hard treatment, that he was obliged to quit England. He went over to New York, carrying with him letters of recommendation from Dr Hutton and Dr Gregory; and he is now a Professor of Mathematics in one of the colleges of that State.

Tired, as I now am of recording examples of Sir Joseph's vexatious, or indecorous, or malignant treatment of *individuals;* let me proceed to alleviate my own fatigue, and probably that of the reader, by adverting, as briefly as possible to his illiberal treatment of different *societies;* after which I shall terminate these remarks.

About the year 1792 or 1793 was established the "*Society for improving Naval Architecture.*" The very title of this Society declares the momentous nature of its objects, especially in a country like ours, which owes so much to its commercial and naval preeminence. The members of this Society were very numerous, and highly respectable in character, consisting principally of public-spirited noblemen, practical engineers, mathematicians, naval officers, merchants, and ship-builders. Their attention was directed to the variety of topics included in the theory and practice of shipbuilding; such as the strength and strain of materials, their preservation, the resistance given by water to bodies of different shapes moving in it, the structure of masts, the shape and position of the sails, the form and operation of the rudder, &c. They made many experiments, and some of them most useful in their tendency: every thing went on well, till their operations began to be impeded by the jealousies of Sir Joseph Banks. Not satisfied with being at the head of the Royal Society, he was anxious to be at the head of this Society also. Sir Joseph was Vice-President; but the President was the late Earl Stanhope, a man of extraordinary talent (whatever his peculiarities might be), and too inflexible to yield to the ambition of one for whose abilities he entertained a most sovereign contempt. Sir Joseph, however, uniformly thwarted the plans proposed by his lordship and the bulk of the Society, and soon formed a party of his own for the purpose of systematic annoyance. This led to a determination on the part of the main body to free themselves from this source of vexation. A series of resolutions was framed, proposed by the late Mr *William Nicholson* (Editor of the Philosophical Journal), and carried by a large majority. In these the Society firmly declared their determination to support the President, so long as he continued to aid and sanction the legitimate objects of the Society. Sir Joseph shortly after retired from the institution; *but not to remain in inactivity.* At that period a most virulent spirit of political animosity, engendered by the acts of the French revolution, was in constant operation. Sir Joseph availing himself of this, and of the political character of Lord Stanhope, most diligently insinuated among different members of His Majesty's government, that the Society, under colour of an association for better purposes, was a jacobinical confederacy with *Citizen Stanhope* at their head. This calumny soon produced its designed effect; so that by a series of rapid steps, which I need not here detail, this useful institution became extinct.

Mr Nicholson afterwards became a neighbour of Sir Joseph's, in Soho Square, and a certain degree of intimacy, in consequence, subsisted between them; Mr Nicholson occasionally conducting experiments on voltaism, &c. at the President's house. Still, his crime in opposing Sir Joseph in the Society for improving Naval Architecture was never cordially forgiven; nor was he ever admitted a fellow of the Royal Society. No, no: "*To be sure Nicholson is a clever fellow. But you know he is only a sailor-boy turned schoolmaster; and we cannot, with any sort of propriety, admit such people among* us.

When the "*Royal Institution*" in Albemarle Street was established, it commenced under too powerful auspices for Sir Joseph to think it expedient to attempt any formal opposition. He therefore became its friend; and favoured the managers with his advice. But his conduct soon proved that he was actuated by the puerile jealousies of a little mind. The influence of the President of the Royal Society might naturally have been employed in recommending to the Institution lecturers of eminence in different departments, or in lending his mature and comprehensive judgment in the formation of a library. But instead of these, Sir Joseph exerted the energies of his mighty mind—how? *in taking care that the Journals of the Royal Institution should not be printed in quarto!* Why not in *quarto?* Truly for this cogent reason, that the *Philosophical Transactions* are published in quarto; and to print those journals in the same sized page might excite an unfavourable comparison!

In 1813, when several distinguished mineralogists and geologists established the "*Geological Society*," the members invited the President of the Royal Society to join them, and he accepted the invitation. He had not long, however, joined this new association before he began to show, as on all similar occasions, that he regarded the Royal Society as the "Aaron's rod" of scientific institutions which was to swallow up all the rest. When the papers read in the Geological Society had so increased in magnitude and interest, that it became a question whether the Society should not issue a volume, Sir Joseph proposed that they should be inserted in the Transactions of the Royal Society. This might perhaps have been acceded to; but it was accompanied with the further extraordinary condition, that *after the Council of the Geological Society had determined what papers should be printed and what laid aside.* "HIS *Council,*" *that is, the Council of the Royal Society, should have the* *further power of adopting or rejecting those which were thus selected for their use.* This strange condition naturally caused the rejection of Sir Joseph's proposal. Immediately he seceded from the Geological Society, with many of his friends. The Society, however, continued to flourish notwithstanding; and in the course of three or four years, several of the seceding members begged to be re-admitted.

In 1818, it was attempted, under the auspices of the British Government, to carry into effect a plan for determining with considerable accuracy the relative values of the weights and measures of all trading countries. This plan originated with *Dr Kelly*, of Finsbury Square, who was about to prepare a new edition of "The Universal Cambist," a comprehensive work on the monies, weights, and measures of all countries, which had been liberally patronized by Government. The Doctor pointed out to "the Board of Trade" the advantages that would accrue to the commercial world, if Government would avail themselves of this season of universal peace, and obtain accurate standards of the principal measures of all countries. In consequence of this suggestion, Lord Castlereagh, by the recommendation of the Board of Trade, issued a circular in March, 1818, directing all the British Consuls abroad to send home copies of the principal standards of weight and measure, employed within their respective consulates, verified by the proper authorities, and accompanied by explanatory papers. This order was executed in the course of the year, in a very complete and satisfactory manner; and the standards thus transmitted were deposited in our Royal Mint, where the comparisons were to be made by *Robert Bingley*, Esq. the King's Assay-master, in conjunction with Dr Kelly; the latter of whom was to publish the results in the new edition of his Cambist.

The business, it would seem, had proceeded thus far unknown to Sir Joseph Banks, although he was a member of the Board of Trade. No sooner did he learn what had been going on, than he expressed great displeasure that the plan was not under *his* direction; and he had sufficient influence to obtain an order compelling the officers at the Mint to desist from all further proceedings in reference to these foreign standards. Thus he occasioned a delay of more than a year. At length, different members of the Board of Trade, who were apprized of this delay, called upon Sir Joseph to assign his reasons, which appeared so frivolous that he was immediately outvoted upon the question, and the inquiry ordered to proceed, agreeably to the original directions. Sir Joseph's plea for his opposition to so natural a plan, was, that as he had recently got Lord Stanhope's scheme overthrown, and a commission appointed to revise British weights and measures, a commission at the head of which *he* was placed, it would be highly unbecoming and indecorous to allow any experiments on *foreign* weights and measures to be carried on at the Royal Mint independently of him and his colleagues in the said commission. This childish pretext was altogether disapproved by most of the members of the Board, and subjected Sir Joseph to the mortification of defeat; a mortification which he did not very long survive.

At the commencement of the present year 1820, several promoters of astronomy in theory and practice instituted an "*Astronomical Society.*" An interesting address explanatory of the objects of the Society, was drawn up and actively circulated, not merely among the friends to astronomical science in Britain, but among the principal encouragers of astronomy on the continent. Stimulated by this address, nearly all the distinguished observers, and other promoters of astronomy in theory and practice in Britain, became members, and several of the most eminent astronomers abroad have testified their approbation of the Society, and requested to be admitted as associates. Among the officers for conducting the affairs of the Institution during the first year, we observe the following well-known names: *Sir W. Herschel, the Astronomer Royal, Drs Pearson* and *Gregory, Col Beaufoy, Capt. Colby, Messrs Babbage, Baily, Colebrooke, Groombridge, J. F. W. Herschel, Harrison, Moore, Stokes,* and *Troughton.* Thus far the Society has proceeded with unpredicted success; and there is every probability that it will long and extensively tend to the diffusion of astronomical knowledge. It has already operated as an incentive to other learned bodies; of which a most gratifying evidence presents itself, in the resolution of the University of Cambridge to erect a new Observatory upon a grand and noble scale.

Copies of this address, so well received in every other quarter, were presented to Sir Joseph Banks: it will be natural to inquire, How did *he* receive them? I really regret to say, that their appearance called forth the most puerile and pitiable jealousy. Several of the members of the new Society were fellows of the Royal Society, and personal friends of Sir Joseph. These he subsequently treated with coolness and incivility; and some he even affected *not to know.* Shortly afterwards he summoned the Council of the Royal Society ("*his* Council," as they were still denominated), laid before them this address, and with a countenance half tinged with melancholy, half with anger, asked what was to be done? They recommended him to let the new Society alone, unless he wished to establish it on a firm and durable basis by his opposition; but this was advice too judicious and sensible to be followed. He then expostulated with the Astronomer Royal upon his grossly reprehensible conduct, in sanctioning the new Society; but the Astronomer Royal was inflexible, and so stupidly blind as not to be convinced by Sir Joseph's arguments, that to sanction a British Astronomical Society was altogether irreconcileable with the duties of Astronomer Royal of Britain. Sir Joseph next expostulated with the *Duke of Somerset*, who had consented to be president of the Astronomical Society, and who, if I am correctly informed, had actually attended one of the meetings, on the incompatibility of this procedure with the duty of a faithful member of the Royal Society: his Grace, in consequence, withdrew from the new Society.

An active member of the Astronomical Society, whose name I need not specify, had received a promise from Sir Joseph Banks, that whenever there was a vacancy among the Royal Society members of the Board of Longitude, he would recommend him as an admirably qualified person. On the death of Gen. Mudge, this gentleman called upon Sir Joseph Banks and reminded him of his promise. Sir Joseph replied, that *by becoming a member of the Astronomical Society he had forfeited all claims upon his recommendation for the Board of Longitude!*

How utterly repugnant all this is to the character of an English gentleman, to say nothing of a philosopher, must be evident to every one. Nothing, it seems, that had a tendency to promote British science in *any* department, could be tolerated by Sir Joseph Banks, unless it were in league with, or rather in subserviency to, the Royal Society: nothing was to be encouraged *by* that Society unless it met his full approbation: and scarcely any person could obtain admission for his papers into the Philosophical Transactions (however indisputable their merit), or for himself as a *fellow* (however established his reputation,) unless either by dancing attendance with assiduous frequency, or, by getting some approved friend to sue for his excuse from this degrading process, he caught the smiles and the sanction of the great man. If genuine science and philosophy have gained ground in England, during the last 40 years, notwithstanding the tendency of these things to impede their progress, it must have been from the operation of circumstances over which Sir Joseph, his habits and propensities, had no control.

After the preceding enumeration of particulars, I may safely ask, If the knowledge of pure mathematics have declined in England during the last 40 years, who can help ascribing it to Sir Joseph Banks? If mixed mathematics, if practical mechanics, if geology and mineralogy, if astronomy, nay, if chemistry, have made any considerable advances in England; or if any of them have been enriched with noble inventions and brilliant discoveries, who will venture to impute the least portion of those advances, or the least valuable of those discoveries, to the fostering influence of that celebrated individual? The flatterers of Sir Joseph have termed him the *Solon*, the *Nestor*, the *Mecænas* of British science; and he must doubtless have laughed in his sleeve at their simplicity or their folly, whenever he knew them so to speak. The MECÆNAS truly! What tragedies did he compose? What memoirs of distinguished men? What classifications of precious stones? What was the name of the learned man whose estates he redeemed, as Mecænas did those of Virgil? What poet or philosopher owed deliverance from royal disgrace to his unsought intervention? What real promoter of science did he on his death-bed recommend to the especial patronage of his Prince, as Mecænas did Horace to Augustus? The MECÆNAS! Into what will flattery precipitate men! Why not the NEWTON, at once? and why not propose for his epitaph—

> **Sibi gratulentur mortales,**
> **Tale tantumque extitisse**
> **Humani generis decus!**

One remark more, and I have done. Much has been said of Sir Joseph's hospitality; and there has been inferred from it the necessity that his successor in the chair of the Royal Society should be a man of opulence. This is very fallacious, and easily exposed. Had Sir Joseph, instead of aspiring to honour among philosophers, contented himself with moving in the sphere of a country gentleman, he would doubtless have given as many dinner parties to the neighbouring gentry, and placed before them as splendid repasts, as he did to his philosophical associates. And as to his *tea-parties*, I know of nothing peculiar about them, except it be that in order to show a philosophic indifference to the Christian Sabbath, they were for many years held on *Sunday evenings.* Several persons who have attended them frequently, and who were, on the whole, pleased with the society they there met with, have always found it difficult to suppress a smile when they have heard of the *opulence* necessary to continue the practice, and affirm that they would engage to defray the whole expense for much less than 150*l.* per annum.

After all, it does not appear quite *axiomatic*, that genuine science and philosophy are essentially, if at all, promoted by this kind of parties. They tend wonderfully to the formation of distinct coteries. When from 50 to 100 persons, especially men of talent, meet frequently, talk much about their own concerns, or speculations, or discoveries, or supposed discoveries, and little about those of other persons, it requires much more watchfulness, and much more freedom from vanity, than usually falls to the lot of mortals, to preserve these individuals from thinking themselves "THE WISE, *and that wisdom will die with them.*"

Doubtless the researches and discoveries of *Hutton*, *Maskelyne*, *Landen*, *Waring*, *Herschel*, *Young*, *Davy*, *Wollaston*, *Mac Culloch*, *Brewster*, *Ivory*, and very many others, the glory of British science, would have had all their intrinsic excellency and all their distinguished celebrity, although Sir Joseph Banks had never gratuitously dispensed a single cup of tea.

ART. XLII.—*Observations and Experiments on the Structure and Functions of the Sponge.* By ROBERT EDMOND GRANT, M.D., F. R. S. E., F. L. S., M. W. S., &c.*—[*Edin. Phil. Jour.*]

SPONGES are aquatic productions; and as the three known species of fresh-water sponge, the *Spongilla pulvinata*, *Spongilla friabilis*, and *Spongilla ramosa*, are now excluded from the genus Spongia by the best authorities, Lamarck, Lamouroux, and Cuvier, all the known species of true sponge are inhabitants of the ocean.

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* Read before the Wernerian Natural History Society, 19th February and 5th March, 1825.

Sponges have a very wide geographical distribution. They have been met with on the coasts of Norway, New Holland, North America, Otaheite, and most of the intervening shores. Fabricius found several species on the shores of Greenland, and nearly an hundred species were brought by Peron from Australasia; so that this genus has a known distribution over nearly 90 degrees of latitude, and within this range they are known to abound on the shores of Europe, Asia, Africa, and America.

Their growth and distribution, like those of other animals, are influenced by climate. They arrive at greatest perfection within the tropics, and become smaller, more rare, and of a firmer texture, as we approach the polar circles. Those most valued in the arts, the *Spongia communis*, *Spongia lacinulosa*, and *Spongia usitatissima*, are inhabitants of the coasts of America, the Mediterranean, the Red Sea, and the Indian Ocean. The small *Spongia compressa* and the *Spongia ciliata* thrive on the frozen shores of Greenland, beyond the 60th degree of north latitude.

It is not known under what pressure of the ocean these delicate creatures may live, but they are found equally in places covered perpetually by the sea, as in those which it leaves dry at every recess of the tide. They adhere to and spread over the surface of rocks, Thalassiophytes, and marine animals, and are so firmly attached to them, that they cannot be removed without lacerating or injuring their bodies. Although they thrive best in the sheltered cavities of rocks, they come to maturity in situations exposed to the unbroken fury of the surge; but in the latter situations, I have always observed them smaller and firmer in their texture, like those inhabiting colder climates, whether their exposure were to the north or south. They cover the nakedness of cliffs and boulders, they line with a variegated and downy fleece the walls of submarine caves, or hang in living stalactites from the roofs.

They attach themselves indiscriminately to bodies belonging to the mineral, vegetable, and animal kingdoms, and the individual species seem to have no law with regard to the particular substances to which they adhere. I have found sponges adhering to basalt, greenstone, sandstone, soft slate-clay, various kinds of fuci, shells of living and of dead *mollusca*, shells of living crustaceous animals, *Sertulariæ*, *Flustræ*, *Corallines*, and even to the soft surface of living compound *Ascidiæ*; but I have not seen them on clay, sand, gravel, bituminous shale, decayed wood, nor any metallic body, though favourably placed to receive them.

The branched species I have always found hanging perpendicularly from the under and sheltered surface of solid overhanging cliffs, or tabular masses of rock. The flat spreading species with projecting *papillæ*, I have generally found on the sides of boulders; while the flat species, without prominent papillæ, are generally seen on the under surface of rocks, or enveloping bodies which admit of a little motion by the agitation of the sea. The delicate and beautiful *Spongiæ compressæ* I have found hanging from the surface of *Ascidiæ*, or of flat sponges, which covered the under surface of rocks: they hang either single or attached in groups, and are likewise seen attached to the rock itself. The *Spongiæ coalita* is a branched species, capable of rising erect, from the great breadth of its base, the firmness of its skeleton, and the frequent anastomoses of its branches.

The known range of magnitude of the sponge is perhaps greater than that of any other animal; indeed, it is one of the most irregular of organized beings in magnitude, form, and colour. The *Spongiæ compressæ* of the Frith of Forth sometimes do not exceed half an inch when full grown, while the splendid *Spongiæ crateriformes* of tropical seas, are known to acquire a magnitude of nearly four feet. And probably sponges acquire a much greater size when they spread over the sheltered surface of cliffs in the warmer parts of the ocean. The flat spreading species of the Frith of Forth acquire a much greater size than any of the branched kinds.

The forms which this animal assumes are very various, and that of the spreading flat species depends much on the surface to which they adhere. They are found sessile, pedunculated, flabelliform, infundibuliform, tubular, foliaceous and branched. From these seven general varieties of form, they have been divided by Lamarck into seven groups, each of which contains many species, again characterised by particular forms, subordinate to the general form of the group. The colours are likewise employed to discriminate the species; but as they entirely change by drying, they afford less useful characters. The most frequent colours are brown and yellow, though sponges are likewise met with grey, green, and white; even red and black species (*Spongia rubra* and *Spongia nigra*) are found near Suez in the Red Sea. The odours of some sponges are decidedly animal, while others belong to common and well-known vegetables. The *Spongia coalita*, when newly taken from the water, smells very strongly of the common muscle, and when burnt, it still resembles the same bivalve burnt; the *Spongia compressa*, on the other hand, smells strongly of the common mushroom; some, as the *Spongia oculata*, have scarcely a perceptible odour.

In their natural state, they are soft and elastic, and possess lively colours; but many of the species, by drying, become quite friable, lose their fine shades of colour, and become white. Soon after death, they pass through a bluish colour to black, by putrefaction. The whole body of a sponge is specifically heavier than sea-water; and each of its parts taken separately sinks in that element.

From the softness and permanent elasticity of the fibres of many species, both in the moist and dry state, this animal is employed in the arts, and in domestic economy, where its place could not be supplied by any other known substance. Books have been written in the German and English languages by Zeller and White, on the uses of the sponge in surgical operations. The fibres contain muriate and carbonate of soda, iodine, and, like Thalassiophytes, carbonaceous matter. From their iodine, they have been much and successfully employed in the treatment of bronchocele, and in the removal of chronie enlargements of the glands, in the mountainous districts of the Continent. In the British dominions, the *Spongia officinalis* (*Spongia lacinulosa*, Lamarck) is recommended as an article of Materia Medica by the Colleges of Physicians of London, Dublin, and Edinburgh. Sponges form a valuable article of commerce in the East, and some islands of the Mediterranean export no other article of trade.

The species of this animal were studied,—their characters were described,—their phenomena in the living state were observed,—and their uses in the arts were known, by the Greeks more than 2000 years ago. It is pleasing to observe, that our forefathers, at such a remote period, were occupied, like ourselves, among the rocks of the sea-shore, experimenting on this humble and apparently insignificant being; and, if we possessed entire the writings of the naturalists of that period, particularly of those alluded to by Aristotle on the shores of the Toronian Gulf, there would, probably, be nothing new in the details of the following inquiry. Aristotle gives an account, in his usual brief manner, of the theories which had prevailed among naturalists before his time, concerning the vitality of the sponge; and mentions the arguments which had been used to prove that this animal is sensitive. It was asserted by some, that the sponge contracted itself, when an attempt was made to tear it from the rock; and that it embraced more firmly the rock to which it was attached, when the winds blew violently upon it, or the waves dashed against it. But he does not state this as his own opinion; and he even mentions, that the naturalists of Torona doubted the truth of these facts. He gives an account of the different kinds of small animals found in the cavities of sponges, some resembling the larvæ of moths, or other winged insects; others resembling earth-worms. He particularly describes the *Pinnopholaces* (*Pinnotheres* of Latreille) or small pea-crabs, as infesting the cavities of sponges; and says, that they open their mouths, to allow small animalcules to enter, and shut them in, to destroy them when caught. These different animals, however, he states, merely shelter themselves, and are nourished in the cavities of the sponge; they become the prey of small fishes when the sponge is torn from its seat; and even the broken remains of the sponge are consumed by the same fishes. But he mentions, that the remains of a sponge thus torn from its place may continue to grow upon the rock, and become a complete sponge; that sponges, in deep and sheltered situations, have a softer texture than in places exposed to the winds and tempests, which check their growth and render them harder; and that they grow best near the shore, in situations, however, where they may be completely covered at high-water, because they are thus washed and cooled by every access of the tide. He speaks strongly of the injurious effects of high temperature on these animals; and says, that it causes them to run rapidly into putrefaction. This remark of Aristotle I have found of service, in keeping sponges alive for the following experiments. The species found on the coast of Lycia, he states, were of great magnitude, but of a rare and loose texture. He gives a description of three different species of this animal, which he characterizes by the looseness or compactness of their texture. Those which are compact, he says, are generally softest; and he mentions, that the helmets and sandals of the Grecian warriors were lined with a strong compact kind of sponge. From an expression which he uses, he even insinuates, that the helmet of Achilles, the hero of the Iliad and of the Trojan war, was lined with the same substance: thus giving the employment of this extraordinary animal, in the arts, an antiquity of 3000 years.

From such a remote origin of the study of this animal, its natural history is now greatly advanced; for we are at present acquainted with nearly as many species of the sponge as of any other animal in the creation. Independent of the subordinate varieties under each species, which amounts often to three, sometimes even to four, as in the *Spongia pala* of Lamouroux, Linnæus has described 14 distinct species; Pallas 27, Gmelin 45, Lamarck 138, and Lamouroux 163 species of marine sponge. Guettard even divided the species into seven great genera; *Eponge*, *Mané*, *Trage*, *Pinceau*, *Agace*, *Tongue*, and *Linze;* he ransacked the ancient strata of the earth in search of antediluvian species, and has described many fossil sponges, which before had been mistaken for fruits or accidental mineral formations. But the philosophy of the sponge, the immutable foundations on which scientific discriminations of the species ought to rest, the minute investigation of the mechanism, the composition, and the uses of all the parts of this animal, and of the extraordinary phenomena it exhibits in the living state,—its mode of growth,—its kind of food,—its habits and diseases,—the means of cultivating an animal, which has so long rendered important services to mankind,—its mode of propagating the species, and extending them over the globe, and the great purposes which it is destined to fulfil in the universe, have remained where Aristotle left them; or rather, in this branch of the study, mankind have gone backward ever since his time; for Pliny, who wrote 400 years after him, conceived, that male and female organs of generation were placed separately on different sponges, although it had been known to the earliest naturalist, that this animal remains immoveably attached to one spot through life, without locomotive power in any of its parts. So late as the year 1752, Peyssonell communicated to the Royal Society of London, as the result of his extensive researches on the splendid marine sponges of the coasts of America, a detailed account of the formation of these substances, by numerous small worms found in their cavities. He says, that these worms construct the sponge like a bee-hive, for the purpose of protection and nourishment; and even that the same kind of worms construct different species of sponge. If the plates which accompanied the writings of Aristotle, and to which he sometimes refers by signs in his description of parts, should hereafter be discovered among the ruins of antiquity, we will there find represented as occasional inhabitants of the sponge, the same worms which Peyssonell, 2000 years after him, mistook for the fabricators of that substance. We now know, that the Nereis alluded to by Peyssonell, infests almost every other soft zoophyte as well as the sponge.

The celebrated Lamouroux, the latest, the most useful, and the most scientific writer on these animals, considers sponges as living masses, without organization, or apparent motion, without a mouth, or organs, or any thing, in fact, which we observe in other animals, and says that this hypothesis is less problematical than any other, and is supported by observations made by him on the sponges of the coast of Calvados in Spain. This statement, from the present illustrious Professor of Natural History at Caen, and author of the most complete and valuable work on zoophytes that has yet appeared, and statements as erroneous concerning the marine sponge from Lamarck, Cuvier, and other eminent modern naturalists, have induced me to push the inquiry to some length, with regard to a very few species; and, though these observations have been made in the depth of winter, and only on the small sponges of the Frith of Forth, they have enabled me to correct some mistakes, and to suggest some new views regarding these animals, which may be useful to those who are interested in such inquiries.

Marsigli, after much research into the nature of marine plants and zoophytes, was convinced that the sponge is a vegetable, and is nourished like Thalassiophytes, by absorbing water through the holes on its surface; and, notwithstanding that he entertained such an opinion of its nature, he was the first who declared, in 1711, that he saw a systole and diastole of certain round holes on its surface. This extraordinary and inconsistent statement, made, for the first time, after this marine production had been studied for upwards of two thousand years, soon spread with the fame and writings of its author, through Europe, and was well known to our countryman Ellis, before he began to investigate the nature of the sponge. The statement which Ellis communicated to the Royal Society of London, in 1765, is a memorable example of the influence of imagination over our very perceptions. This great zoophytist, having his mind already prepossessed by the assertion of the Italian naturalist, placed living portions of the *Spongia urens* and *Spongia cristata* in glasses of sea-water, when on the coast of Sussex, and declares that he not only saw the contractions and dilatations of the apertures mentioned by Marsigli, but likewise saw the water pass to and fro through the same apertures on the surface. Pallas immediately copied this hasty assertion from the Transactions of the Royal Society, into his Elenchus Zoophytorum, which he published the following year. Other naturalists followed so great an example, and thus was established and propagated an important error, which has pervaded the works of zoologists for half a century, and greatly retarded the progress of this interesting branch of comparative anatomy.

Before examining the structure of the sponge, I had frequently seen, with the assistance of a microscope, the currents established in sea-water, by the rapid vibration of the ciliated tentacula of many Polypi, particularly of the *Sertularia* and *Alcyonia*, and likewise by the ciliated circular margins of such as possessed no tentacula. And, as every writer since the time of Marsigli agreed in considering the round apertures on the surface of the sponge as the passages through which nourishment is conveyed to the animal, I naturally began by a careful examination of these canals, in a variety of sponges. But, instead of finding any ciliated margins, or distinct polypi, within these large tubes, or any apparatus sufficient to create a current into them, I found them, particularly in the *Spongia panicea*, where they are wide and distinct, lined throughout their whole winding and anastomosing course, with a smooth, soft, glistening membrane. This transparent colourless membrane was very evident at the angles of separation between the branches of the internal canals; for at these places few of the horny fibres shot into it, and it could there be raised by the point of a needle; but, throughout the rest of the tube, it appeared stretched from fibre to fibre, and so firmly connected with the axis or skeleton of the animal, that it could not possibly contract so as to empty the whole of the internal canals, without a general contraction of the entire sponge. But as I knew already that the animal never contracted its body, nor could be forced to do so by the strongest irritants, I found it impossible to explain the power of suction ascribed to these canals by any theory; and, in this dilemma, I had recourse to the microscope, well assured, that if currents really passed to and fro through the round apertures, they might be seen by the same means which had so often detected the currents of much smaller zoophytes.

In the month of November last, I therefore put a small branch of the *Spongia coalita*, with some sea-water, into a watch-glass, under the microscope, and, on reflecting the light of a candle up through the fluid, I soon perceived that there was some intestine motion in the opaque particles floating through the water. On moving the watch-glass, so as to bring one of the apertures on the side of the sponge fully into view, I beheld, for the first time, the splendid spectacle of this living fountain vomiting forth, from a circular cavity, an impetuous torrent of liquid matter, and hurling along, in rapid succession, opaque masses, which it strewed every where around. The beauty and novelty of such a scene in the animal kingdom, long arrested my attention, but, after twenty-five minutes of constant observation, I was obliged to withdraw my eye from fatigue, without having seen the torrent for one instant change its direction, or diminish, in the slightest degree, the rapidity of its course. I continued to watch the same orifice, at short intervals, for five hours, sometimes observing it for a quarter of an hour at a time, but still the stream rolled on with a constant and equal velocity. About the end of this time, however, I observed the current become perceptibly languid, the opaque flocculi of feculent matter, which were thrown out with so much impetuosity at the beginning, were now propelled to a shorter distance from the orifice, and fell to the bottom of the fluid within the sphere of vision; and in one hour more the current had entirely ceased.

The following morning, I separated, with great caution, from the rocks, a variety of flat and branched sponges, and examined their currents through the microscope, with a candle, in a darkened apartment, which is certainly the mode of seeing these currents best through the double reflecting microscope. The currents of water were distinctly visible in every species of sponge which I examined; and, even where the apertures were scarcely seen by the naked eye, the microscope shewed a powerful current issuing from them. In all the specimens, the currents were seen to flow continually from the apertures, however long they were observed; and the discharge of excrement which always accompanies the stream never fails to make its appearance, even though the purest water is employed. The velocity of the stream, in the same species, corresponds very much with the entireness of the branch employed, its recentness from the sea, and the fewness or smallness of the apertures in a given space. The branched species shew the currents best by this mode of examination, because they can be adjusted to the microscope with least mutilation of their bodies, and are most conveniently managed from their smallness and lengthened form. The *Spongia coalita*, which has a glistening or membranous surface, with very few apertures, exhibits a powerful stream; while the *Spongia oculata*, *Spongia xerampelina*, and *Spongia palmata*, which have a more open woolly surface, propel their discharge to a shorter distance from their numerous circular apertures.

On attempting to examine some of the larger flat species, as the *Spongia panicea* and *Spongia cristata*, in the same manner with the microscope, I found it not so practicable. The dissection necessary to reduce them to a smallness fit for examination under that instrument, and in a watch-glass, threw open the canals so much as to destroy their means of manifesting a concentrated current. But a single papilla torn from a *Spongia papillaris*, or other spreading sponge, which has the papilla much elevated, shows distinctly the current flowing constantly from it, when placed in a watch-glass with sea-water under the microscope.

From the distinctness of the currents seen by this means, I was induced to try whether they might not be perceived by the naked eye, although they had escaped the observation of so many naturalists, and particularly of Montagu, who states, in the Memoirs of the Wernerian Society, that he could not perceive these currents, even with the assistance of a glass. On placing a fresh placentiform mass of the *Spongia panicea* in a glass of clear sea-water, I could distinctly perceive, with the naked eye, particles propelled to the surface of the water from a large circular orifice in the centre of the mass. I afterwards placed a portion of the *Spongia cristata* in a shallow vessel, and covered it to the depth of half an inch with water. The orifices along the ridges at that distance from the surface, created a current there visible to the naked eye, and particles of dust floating on the surface of the water were drawn into the stream, and carried to the distance of two or three inches from the apertures. I suspended a *Spongia compressa* by its pedicle, in its natural position, with its aperture downwards; and even in this small animal I could perceive a languid current issuing, and opaque particles occasionally propelled. The currents from this species are best examined by the microscope, but still are comparatively languid: this sponge has a villous surface, and very large apertures, which are circumstances generally accompanying a weak current.

The *Spongia panicea* presents the strongest current which I have yet seen, and has the greatest thickness of body of any spreading sponge which I have met with on the rocks of this part of the Frith of Forth. Two entire round portions of this sponge were placed together in a glass of sea-water, with their orifices opposite to each other, at the distance of two inches; they appeared to the naked eye like two living batteries, and soon covered each other with feculent matter. I placed one of them in a shallow vessel, and just covered its surface and highest orifice with water. On strewing some powdered chalk on the surface of the water the currents were visible at a great distance, and on placing some small pieces of cork or of dry paper over the apertures, I could perceive them moving, by the force of the currents, at the distance of ten feet from the table on which the specimen rested. A portion of soft bread, pressed between the fingers into a globular form, with a diameter larger than that of the orifice, and placed over it, was not moved away in a mass by the stream, but was gradually worn down by the current beating on its sides, and thus propelled to a distance in small flakes. A portion of unburnt black-coal, with twice the diameter of the orifice, was instantly rolled off the mouth of this living fountain, in whatever position I attempted to make it rest upon it. A globule of mercury, of equal diameter with the orifice, let fall upon it through a glass tube was not removed or shaken, and completely stopped the current. I now pierced, with a needle, a thin superficial canal, in the vicinity of the closed orifice, and established a new current, which continued, even after removing the obstruction from the original orifice.

A globule of mercury, of any smallness, placed over the orifice of a living sponge, is too heavy to be affected by the small column of water which impels against its smooth round surface, flowing at the rate with which it issues from that orifice, and is useful in enabling us to stop up the currents of certain orifices, in order to direct the stream with greater force through a particular aperture, which we wish to examine through the microscope. By adopting this plan with a healthy *Spongia panicea*, which has generally very few and large orifices on the surface, we can distinctly perceive, with the naked eye, that the current never enters by the same apertures through which it issues, and we might thus measure the whole strength of the forces employed to produce the currents in any particular specimen.

But as the *Spongia cristata* was the kind in which Mr Ellis imagined he saw the water pass to and fro through the same apertures, I placed, in a glass of clear water, a portion of this sponge, which consisted of a single ridge, with a range of projecting apertures along its vortex, and on examining, for some time, with a pocket lens, each of the apertures in succession, through the transparent sides of the vessel, I found that they all sent forth a constant and uninterrupted stream of water from the interior of the animal, with occasional discharges of opaque feculent matter. In order to be still more satisfied that none of the apertures were destined to receive or inhale water, while others propelled it from the animal, I placed under the microscope a small branch of the *Spongia coalita*, on which there were only three apertures, and I found that, however long I watched them, they were always employed in conveying water from the interior. But as it was still by no means impossible that, while my attention was fixed on one aperture which discharged water, another, which I had just quitted, might now be inhaling that fluid, I took from the rocks some specimens of the *Spongia compressa*, constructed like a small white paper bag, with only one round aperture at the extremity of the body; and on placing each in succession under the microscope with sea-water, I found that these animals sent forth from the only aperture of their bodies a slow but constant stream.

It thus appears, that the round apertures on the surface of a living sponge, are destined for the conveyance of a constant stream of water from the interior of the body. This stream conveys away the particles of excrementitious matter constantly separating from the interior of the canals, and which are not only seen, by the assistance of the microscope constantly issuing from the apertures, but are even perceived by the naked eye propelled occasionally in flakes; and when a portion of any kind of living sponge is allowed to remain for a day at rest, in a white vessel filled with the purest sea-water, accumulations of feculent matter are always seen immediately under each orifice. The feculent matter of the *Spongia palmata*, *Spongia oculata*, and *Spongia xerampelina*, consists of a very fine dark-brown dust: while in the *Spongia panicea* and *Spongia cristata*, it consists of larger flocculi of a dark-grey membranaceous substance. The streams likewise convey from the interior of the animal, along with the excrements, certain soft, round, small bodies, generally of an opaque yellow colour, which are distinctly seen disseminated through the whole texture of most marine sponges, and which, for the present, we shall consider as the ova. The round apertures may, therefore, without impropriety, be termed *fecal orifices*, in order to distinguish them from the *pores* of a very different nature, which are destined to transmit water into the interior of the body.

The fecal apertures are raised to the extremities of projecting papillæ, in such sponges as cover the sides of rocks, in order to convey the excrements beyond the pores and general surface of the animal. In the *Spongia oculata*, *Spongia* *palmata*, *Spongia xerampelina*, and such branched species as have a soft downy surface, the fecal orifices are ranged in close order along the outer margins of the branches, and very few are observed on the flat surface, in order to prevent the excrement from falling in the direction of the flat woolly surfaces, which would be very apt to retain it, and thus choke up the groups of pores which are seen every where over their surface. Such branched sponges have not, and do not require projecting papillæ, because they hang suspended by a narrow stem, and are kept sufficiently clean by receiving gentle undulations from the constant motions of the sea. The same applies to the soft downy white *Spongia compressa*, which always hangs down, and whose orifices are always marginal. The bright yellow porous placentiform mass of the *Spongia panicea* has no papillæ; indeed the fecal orifices are sometimes even lower than the general surface of the animal, and I have never seen this sponge, excepting on the under surface of rocks, with its orifices perpendicularly downwards; so that the excrements fall clear of its surface by their own gravity, without the assistance of papillæ. The flat species which are found encrusting Fuci, Sertulariæ, Corallines, or other moveable bodies, have very seldom prominent papillæ, because they are cleansed by the agitations of the sea like the branched sponges.

What relates to the cleanliness of their surface applies equally to their means of receiving food; for it is only in proportion to the free entrance of water, through the minute pores on its surface, that these animals enjoy health or existence; and we shall find the arrangement, form, and structure of these pores, beautifully calculated to prevent the entrance of all grosser particles into the interior of the canals.

Mr Ellis was probably led to imagine that the currents passed to and fro through the same apertures, and thus to compare these openings to Polypi, by observing the irregularity of the feculent discharges, which do not come out in particles, and in a constant stream, but generally in flakes at intervals, in the species which he examined; or he may have been deceived by the appearance of the whirlpools that are generally seen at the sides of the apertures when they are near the surface of the water. But it was of much importance in the physiology of the sponge, to determine precisely the nature and direction of these currents, because the power of sucking in and throwing out water by the same orifices, like Polypi, hitherto ascribed to this animal, necessarily implied the existence of two other properties which it has long been supposed to possess, the power of contracting and dilating its apertures, and that of contracting or shaking its whole body when touched.

(*To be continued.*)

Art. XLIII.—*Essay on Earthenware and Porcelain.*—[*Parkes' Chemical Essays.*]

In attempting the history of the manufacture of European porcelain, it may be remarked, that although porcelain had been brought into this quarter of the world before Christ, as has been mentioned above, there is reason to believe that the introduction of the knowledge of making porcelain in Europe was but of late years, as will be seen in the sequel.* Porcelain is not made even now in the Indies; what is consumed there is all imported either from Persia, Japan, or China, and the other kingdoms between China and Pegu.

The English cannot, I presume, lay claim to much excellence in their manufacture of earthenware before the time of the late Mr Wedgwood, who began his indefatigable researches about the year 1760: for, in the beginning of the last century, all the best tiles and even the finer sort of bricks were imported from Denmark, Germany, and Holland.† Not but that bricks and tiles of clay had long been made amongst us; for there is an act of parliament now on the statute books, that was passed in the reign of Edward IV,‡ which directs the time for digging the earth, the manner of making the bricks, &c.; and it is so expressed, that we have reason to conclude that bricks had been made in England from time immemorial. While on this article, I am desirous of mentioning that there is a paper on the process of burning bricks, in the third volume of Bergman's Chemical Essays, and that it contains many hints which may be useful to those who are engaged in that occupation.

It seems strange that our remote ancestors did not arrive at greater perfection in the manufacture of all these articles, when it is considered that the island produces not only a great variety of clays, but also all the other materials necessary for the production of the best earthenware and porcelain: Dr Martin Lister, who wrote in the latter part of the seventeenth century, distinguishes no less than two-and-twenty different sorts of clay in Britain, which fell under his own observation.* Moreover, the exquisite pieces of workmanship which were produced at different periods by the English sculptors, show that there was no deficiency of genius and skill among our artists, who probably conceived that the working in clay was beneath their attention.

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* Harmer's *Observations*, vol. i. p. 75.

† Houghton's *Collections for the improvement of Husbandry and Trade*, vol. ii. p. 26.

‡ See 17th Edward IV. cap. 4.

The common stoneware, and that called white enamel, have been made in various countries of Europe ever since the fifteenth century. The former of these is distinguished by a glaze made by means of muriate of soda; the latter is covered with a real glass, rendered opaque by the white oxide of tin.† Stoneware is the only article of pottery that can be employed for chemical purposes, where a great heat is required. Macquer says that our best common stoneware is the most perfect pottery that can be, and that it has all the essential qualities of the finest old Japanese porcelain. Mr Pott, the author of *Lithogeognosia*, has written a treatise expressly to explain the requisite perfections of stoneware for the purpose of chemical vessels; but I conceive that what is made at Lambeth near London by the best of those manufacturers, is susceptible of but little improvement. For boiling syrups, and for many other culinary purposes, the Lambeth stoneware is the best article that can possibly be employed; as it is cheap, will stand a great heat, and is perfectly wholesome; whereas many of the earthern utensils used in our kitchens are glazed with lead, and consequently are extremely dangerous. Several years ago, I adverted to this subject in another work; but I was desirous of repeating the admonition now, because I consider the health of the community must be impaired by the frequent use of earthen vessels which are covered with the common Staffordshire glaze. The acetic acid will readily dissolve the oxide of lead; hence the boiling of pickles, and the making of other culinary preparations in such vessels, must be highly improper.

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* *Phil. Trans.* No. 164, p. 256. Campbell's *Political Survey of Great Britain*, 4to. 1774, vol. ii. page 16.

† The oxides of many other metals are used in different branches of pottery, as will be shown in connexion with the latter part of this subject.

The white enamel ware was brought to its present state of perfection by Bernard de Palissy, a native of the diocese of Agen, in the province of Guienne in France; a spot celebrated for being the birthplace of the memorable Joseph Scaliger.*

Palissy was in a low station of life; but he was eminent for his knowledge, industry, and talents. There were indeed so many interesting traits in his character, that I trust I shall be excused if I recite a few of the chief circumstances of his history.

He is said to have been a skilful painter upon glass,† but he was more generally known as a chemist. Originally he was a land-surveyor and draughtsman; but his taste for natural history led him to abandon this employment, and induced him to travel for instruction over the whole of France and Lower Germany. An accidental circumstance threw into his hands a cup of enamelled pottery; and, from that time, his whole attention and fortune were taken up in experiments on enamels.‡ Nothing can be more interesting than the narrative which he himself has given of his labours.

He exhibits himself as building and rebuilding his furnaces, always on the eve of success; worn out by labour and misfortune; the derision of the public; the object of the angry remonstrances of his wife; and then as being reduced to such an extremity as to burn his furniture, and even some of the wood-work of his house, to keep his furnaces going. His workman presses him for money, he strips himself, and gives him part of his clothes. But at length, by dint of indefatigable labour, constancy, and genius, he arrived at the desired degree of perfection, which gained him the esteem and consideration of the greatest men of his age.

He was the first who formed a collection of natural history at Paris. He even gave lectures on that science, and received a moderate subscription from each of his auditors, under the obligation of returning it four-fold if any thing he taught should prove false. He was the author of many singular books on subjects of agriculture, fire, earth, salts, &c. that are now very difficult to be found, and it is to him especially that Buffon is indebted for many useful hints.

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* Scalliger is called *memorable*, because he was not only well versed in all the sciences, but understood thirteen different languages. It has been said that he was the most learned man that any age ever produced. The variety of subjects on which he wrote with applause is truly astonishing.

† In the time of Palissy the art of painting upon glass was nearly lost. In this country it was classed with the ARTES PERDITÆ; but Mr Walpole has shown, by a regular series of artists and their performances, that this secret was never entirely lost. Walpole's *Anecdotes of Painting.*

‡ The modern enamel painting is quite a different art. This was invented by John Petitot of Geneva. An account of his experiments and discoveries may be seen in the Biographical Dictionary, article *Petitot*, and also in Grainger's Biographical History, vol. ii. p. 288.

Palissy was the first who ventured, in the face of the priests, to affirm that fossil shells and calcareous mountains are the remains of real shells; he was also the first who taught the true theory of springs, and was in all respects an eminent and accomplished man. The very form of his works exhibits a proof of original genius; they consist of dialogues between Theory and Practice, in which Practice is always the instructor, while Theory is represented as a scholar proud of his own understanding, but indocile and ignorant.

The high reputation he acquired, and the obligations under which his countrymen stood indebted to him, were however not sufficient to defend him from the persecution of the League; for, being a protestant, Matthew de Lawnay,* one of the greatest fanatics of his day, caused him to be dragged to the Bastile at the age of ninety years, where he signalized himself by acts of firmness and heroism.

His reply to Henry III. deserves to be commemorated. "My good man," says the King, "if you cannot reconcile yourself to the matter of religion, I shall be compelled to leave you in the hands of my enemies." "Sire," said Palissy, "I was perfectly ready to surrender my life; and if the action could have been accompanied with any regret, certainly it must have vanished, after hearing the great King of France say, 'I am compelled.' This, Sire, is a situation to which neither yourself, nor those who force you to act contrary to your own disposition can ever reduce me, because I am prepared for death; and because neither your Majesty nor your whole people have the power to compel a simple potter to bend his knee before the images which he fabricates."† This venerable man died about the year 1590.

Several of the nations of Europe at the dawn of the eighteenth century, could not but regret that the knowledge of the manufacture of porcelain had not been introduced among them. At this time, however, a fortunate circumstance aroused the attention of the French people, and directed it to this important subject.

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* This man must not be confounded with the celebrated De Launay who wrote "Remarks on the Roman Jurisprudence," and died in 1693.

† Chaptal's *Elements of Chemistry*, vol. ii. p. 90; and the *Biographical Dictionary*, article Palissy.

Francis D'Entrecolles, who had resided many years in China as a Christian missionary, a man of insinuating manners, and of a mild and affable deportment, had the address to procure specimens of the materials used by the natives in their porcelain, and these he sent to France with a summary description of the Chinese processes.

It may appear strange that so jealous a people as the Chinese are known to be, should have allowed of European missionaries being settled among them. The ecclesiastical historian. Mosheim has, however, investigated this subject, and it is highly amusing to read his account of the arts which the Jesuits practised to interweave the doctrines of Jesus with those of Confucius, and to learn how they ingratiated themselves with the inhabitants of that country.*

When the specimens arrived in France, the celebrated Reaumur undertook a series of experiments to discover the method of imitating the Chinese productions; and he persevered with amazing industry until he had obtained the main object he had in view.

The difficulties which he had to encounter; the mistakes he made during the investigation; the cause of those mistakes, and the extent of his success, are all well detailed in the French Dictionary of Chemistry, translated by Mr Keir, and will be found under the article Porcelain. Mr Reaumur himself in the years 1727 and 1729 also published a full account of his progress in this inquiry, in two memoirs addressed to the Academy of Sciences, and which were printed in their Transactions.

While Reaumur was engaged in these experiments, porcelain was begun to be manufactured in Saxony, and soon afterwards other manufactories of it started up in different parts of Germany. We had also an establishment for making it at Chelsea; and since that period various companies in different parts of England, France, and Italy, have been founded for the same purpose.

The first person who made porcelain for sale in Saxony was the Baron Botgar, and it is probable that his was the first manufactory of real porcelain in Europe. In order as much as possible to preserve this art as a secret, the works were rendered impenetrable to any but those who were immediately employed in them.*

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*See an octavo pamphlet printed by Tonson in 1760, entitled "Authentic Memoirs of the Christian Church in China."

M. De la Condamine, in his journey into Italy, visited a manufactory of porcelain established at Florence by the Marquis de Ginori, governor of Leghorn, where he saw statues and groups half as large as nature, and modelled from some of the finest antiques. He describes the body of the ware as being equal to the best Chinese manufacture, and that it would be extremely beautiful if it had a whiter glazing; but that the Marquis was determined to use only such materials as were found in his own country. How superior are the advantages which the English nobility possess for improving the manufactures of these kingdoms, if it were thought becoming in them to attend to such objects! In France, the Count de Lauraguais engaged in the pursuit of improvements in porcelain for several years with uncommon ardour and success. *French Chemical Dictionary.*

The establishments of the most consequence among the moderns are, one at Dresden; the King of Prussia's own manufactory at Berlin; and the extensive works which belonged to the late King of France at Sevres,† near Paris. There is also a considerable establishment for the manufacture of porcelain at Montpelier; a descriptive account of which, together with the process for making the peculiar glaze which is employed there, was published some years ago in the *Annales de Chemie.*‡

Frederick the Second, King of Prussia, had conceived so high an opinion of the importance of the manufactory at Dresden, that when he conquered Saxony he took many of the best workmen away by force, and transported them to his own pottery at Berlin.§ This manufactory at Berlin since the year 1763 has been carried on for his Majesty's own private account, with success and with good taste. Five hundred men have constant employment in it, and large quantities of the porcelain are annually exported.||

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* See the *Travels of Jonas Hanway*, in 4 volumes, 4to. London, 1753.

† For an interesting acconnt of the establishment and management of the manufactory at Sevres, see the memoir of Brongniart, the superintendant of the works. *Phil. Mag.* vol. xiii p. 342; and vol. xiv. p. 17, &c.

‡ *Annales de Chimie*, tome ii p. 73—85.

§ Wraxall's *Memoirs of the Courts of Berlin, Dresden*, &c. 2 vols. 8vo. London, 1806.

|| Busching's *Tour;* and the Appendix to the *Monthly Review*, vol. lv. p. 554.

Mr Jonas Hanway, in the account of his travels, has published a particular detail of an immense collection of porcelain which is laid up in the Chinese palace at Dresden. The whole recital would be unsuitable to these Essays; but the following particulars will show how the Elector of Saxony valued himself on account of the perfection to which the manufacture of porcelain had arrived in his dominions.*

"The vaults of this palace," says he, "consist of fourteen apartments filled with Chinese and Dresden porcelain. One would imagine there was sufficient to stock a whole country; and yet they say, with an air of importance, that a hundred thousand pieces more are wanted to complete the intention of furnishing this single palace."

"Here are a great number of porcelain figures of wolves, bears, leopards, &c., some of them as big as the life; a prodigious variety of birds, and a curious collection of different flowers. A clock is preparing for the gallery, whose bells are to be also of porcelain: I heard one of them proved, and sufficient to form any music; but the hammers must be of wood"

"Here are forty-eight large China vases, which appear to be of no use, nor any way extraordinary, except for their great size; and yet his Polish Majesty purchased them of the late King of Prussia at the price of a whole regiment of dragoons."†

In the course of the narrative, Mr Hanway mentions one very interesting circumstance, viz. that samples of the *first efforts* to make china in Saxony were laid up by the King of Poland in this palace, together with specimens of all the improvements, in their several gradations, from the first, till the time when the manufactory arrived at its utmost state of perfection;‡ an idea truly philosophical, and which reflects more honour on his memory than the bartering away the liberties of his subjects for pieces of foreign porcelain.

The passion for collecting porcelain has been common in most countries. Dr Campbell remarks that "to Ormus, at the proper season of the year, resorted the merchants from Persia, Arabia, and Turkey. At these seasons," he says, "the city wore a new face, the outsides of the houses were adorned with cabinets of porcelain, and with beautiful paintings; and all the riches of the East blazed in the eyes of the captivated spectator."*

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* The potters of Dresden were always as curious in the management of their ovens as they are in the manufacture of their porcelain. Nothing but white wood is employed for heating the kilns, and this is never used till it is become thoroughly dry.

† *An Historical Account of the British Trade over the Caspian Sea, with a Journal of Travels*, by Jonas Hanway, 4 vols. 4to. London, 1753.

‡ The potters in Saxony are so exact in the management of their manufactories, that they employ none but the purest rain-water for soaking the clay; and they prepare the clay only twice a year, at the equinoxes; conceiving that at those seasons the water is less contaminated with foreign ingredients.

The greatest part of the earthenware that is now made in England is manufactured in a particular district of about ten miles extent, well known by the name of the Staffordshire Potteries.† Here earthenware has probably been made ever since the time of the Romans;‡ but there did not appear to have been any ambition among the manufacturers to improve the art, or to produce any works of taste, till the late Mr Wedgwood engaged in the business. This most excellent man brought modellers from Italy and from other parts of the continent, whom he engaged at high wages; and he also constantly employed a competent chemist in experiments,|| that nothing might be wanting which could conduce to extend the employment of earthenware throughout Europe, or that could in any way tend to give a permanency to the staple manufactory of the county which had given him birth.

Amidst a collection of the most beautiful medallions of British and Foreign Worthies, the manufactory at Etruria produces vases and urns in imitation of jasper and other variegated stones; a fine black porcelain, of which very beautiful vases and bas-reliefs are made, after antique patterns; Etruscan vases ornamented with encaustic paintings, after the antique; and bas-reliefs of a white composition, on coloured grounds, so as to have the effect of enlarged cameos. To the late Mr Wedgwood the country is indebted for all these discoveries in the art of pottery.

Notwithstanding the energy with which Mr Wedgwood devoted his inventive talents and chemical knowledge, to the improvement of his favourite manufacture, during the course

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* *Political Survey*, vol, i. p. 37.

† Some account of this manufactory may be seen in Plot's *History of Staffordshire;* in Arthur Young's *Six Months' Tour through the North of England*, in 4 vols 8vo. London, 1770; in Aikin's *Account of the Country round Manchester;* and in the *Chemical Catechism*, chap. v. page 110.

‡ In sinking pits very evident remains of Roman potteries have been discovered, and at a considerable depth below the present surface of the land.

|| See the first vol. of these Essays, p. 22.

of a long and industrious life, I have no doubt that chemistry might still confer many benefits on the potter's art, if the manufacturers in general would bestow such an education upon their children as would enable them to pay a proper attention to the cultivation of this science.

Impressed with this idea, I have myself frequently inspected several of the largest manufactories of earthenware in this kingdom, as well as many others of inferior note, and have examined their various processes with the utmost care and attention. A residence of several years at Stoke-upon-Trent, in the midst of the Staffordshire potteries, afforded me the best opportunities for this purpose, and enabled me to make those inquiries at my leisure, and to acquire that information which none but an inhabitant could easily have obtained.

It will not, however, be necessary for me, while giving an account of this manufactory as at present conducted, to go much into detail; because, if this were the object I had in view, it would be necessary to devote a volume or more to the purpose; for I am acquainted with few manufactories in which the operations are more numerous and diversified.

On this account, I shall content myself with giving such an outline of the business as will afford the general reader a correct idea of the nature of the different operations; and at the same time shall propose, for the consideration of the manufacturers themselves, such hints for the improvement of some of the processes, as I conceive to be worth their attention.

Before I proceed to a descriptive detail of the several processes in the manufactory of earthenware, it will be proper to say a few words on the nature of the different materials which are usually employed to form the body of the ware itself.

It is well known that the chief ingredients are clay and flint; the former of which, when pure, is known to chemists by the name of *alumina* and the latter by the name of *silica*; and that no kind of earthenware can ever be perfect, unless it be made of a suitable sort of clay,* and in that proportion only which shall be correspondent to the quantity of flint employed. The experience of ages has confirmed this, and has proved that good pottery, as Vauquelin has remarked, differs from bad, less in the diversity of its elements, than in their proportions. How crude have been the notions of men respecting the chemical nature of bodies! The great Buffon "considered clay to be merely silica attenuated, and, as it were, rotted by the action of water, the air, and the sun."

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* Macquer's Paper on Clays, in the *Memoirs of the Academy for the year* 1758, is highly deserving of perusal.

Baumé was of opinion that sulphuric acid is a component part of all clays, and that the difference which is observable in different samples of this earth is chiefly owing to variation in the quantity of this acid—but this opinion is also erroneous.

There are four kinds of clay in common use in the Staffordshire potteries,* and they are known by the following appellations, viz. *Black* clay, *Cracking* clay, *Brown* clay, and *Blue* clay. The two former are the produce of the south of Devonshire, the others are brought from the Isle of Purbeck in the county of Dorset. With regard to the price of these clays, that of the two first is 14*s.* per ton, delivered on board the vessel, or 44*s.* when delivered in the potteries. The price of the two latter is 21*s.* on board, or from 48*s.* to 52*s.* per ton, when delivered to the works.

The *black clay* takes its name from the colour which it usually bears, and this is derived from a quantity of bitumen or coaly matter which it contains. This carbonaceous portion is, however, entirely consumed when the clay passes through the potter's oven, for the articles made with it become when properly fired of a good white.† It is, indeed, a curious fact, that the blacker this clay is when it is first dug, the more white will the earthenware be, which is made with it.

The second kind, called *cracking*, has acquired its name from a property which it has of occasioning the ware to crack during the first process of burning, or while making it into biscuit, if by accident a larger quantity than usual of it has been employed. But notwithstanding this singular property, the manufacturers are fond of it, on account of its extreme whiteness. It is, however, always dangerous to use it, unless it be employed with judgment, because it has a peculiar tendency to crack even when mixed with the usual proportion of silica. Every kind of clay, when dried without any admixture, will be liable to this inconvenience; because, if *pure* argillaceous earth be made sufficiently soft to be modelled by the potter's wheel, it will shrink one inch in twelve during the process of drying, and this will inevitably produce the effect in question.

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* For the properties of the best porcelain clay consult the French *Chemical Dictionary*, article *Clay*, or Chaptal's *Chemistry applied to the Arts*, vol. iii. p. 225.

† The Roman potters must have learnt that the native clays differ very much in their nature and properties, for they had different names to distinguish them; such as *Argilla* for potter's clay; *Terra pinguis*, black clay; *Leucargillion*, white clay; *Terra sigillaris*, fine clay; and *Tasconium*, the clay for crucibles.

The third kind which I have mentioned, is what is known by the name of *brown clay*. The excellency of this sort is, that it burns white, without danger of its cracking like the last species, although it is liable to another imperfection which is termed *crazing*. Some manufacturers, however, employ this clay largely, while others, on account of this peculiar property, refuse it entirely.

CRAZING is a technical term to denote the cracking of the glaze. It is believed that this generally arises from a defective union of the glaze with the body, owing to the clay, or the mixture of the clay with the other materials being ill calculated to receive the glaze properly. But may it not be occasioned rather by the glaze itself being not perfectly fusible? I suspect, however, that it more frequently originates from the circumstance of the ware being drawn out of the oven before it has time to cool gradually, and for the glass to become properly annealed. If lime be mixed with the materials which form the body of the ware, that will unavoidably occasion the crazing of the glaze.

The fourth kind, called *blue clay*, is considered to be the best, and, indeed, it always costs the highest price. This not only burns white, but it forms a very solid kind of ware, and will bear a larger proportion of flint than any other; and it is well known to potters, that the greater the quantity of silica, the whiter will be the earthenware; though if more of this species of earth should be employed than the clay will admit, it would occasion the goods to crack in the second burning. Both this and the brown clay are now much used for making porcelain.*

It requires much practice to be enabled to understand the nature of clays sufficiently to employ them in pottery to the best advantage. Dr Lewis, in his notes to Neumann's Chemistry, has given the results of many experiments on the mixture of clay with the different earths and earthy compounds for the purposes of making pottery, which may be read with advantage by those who are desirous of investigating this subject.* Chaptal speaks of a white earth from Ayoree in North America, which the French potters employ. Would not this be of equal value to our manufacturers?

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* An interesting enumeration of a variety of other uses to which clay may be applied, will be found in Fourcroy's *System of Chemistry*, vol. ii. p. 207.

Silica, or pure flint, as I have stated above, is likewise an important article in the manufacture of earthenware; forming, in general, a fourth, a fifth, or a sixth part of the whole of the bulk of the prepared mixture or paste, when taken by weight.

There is however one remarkable difference in the properties of silica and alumina, which has a very manifest effect on porcelain. Pure silica is transparent; but the thinnest lamina of alumina is entirely opaque.

Alumina has such an affinity for silica that it will unite with it even in the humid way, and form a kind of mortar, which will harden by time, and be afterwards incapable of decomposition by the action of the atmosphere. This combination is the basis of many of the gems and precious stones.

Much of the flint used in England is brought from the neighbourhood of Larne in the county of Antrim, in Ireland; but as this comes out of a bed of limestone, it is not so good for the potter's use as the chalk flint. The price of the latter is at this time about 35*s.* per ton, delivered in the Staffordshire potteries. The flint which is most approved is procured chiefly from Gravesend, where it is found imbedded in the chalk.† It is usually sold at 40*s.* per ton, delivered at the works.

The English potters procure their flint on these low terms, because the people who are employed in Kent and the neighbouring counties in getting the limestone and chalk, are obliged to raise the flint also; which being in their way, the proprietors of the quarries are glad to get rid of it even at a very inferior price.

It has been suggested by a friend of mine, that an inexhaustible source, for supplying the manufacturers with very excellent flint, would be the sea-shore; and that a person conversant with chemistry might readily examine a few samples of pebbles, and determine which were most suitable for making good pottery.

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* Lewis's Neumann, London, 1773, vol. i. p. 30, &c.

† Silica is also a component part of various other stones, and is the basis of rock crystal, quartz, agate, &c.

For making the finer kinds of earthenware a considerable quantity of Cornish clay is also used, known commonly by the name of China-clay. This is the decomposed fel-spar of the granite; and it is prepared in Cornwall by the clay-merchants themselves, before it is transmitted to the Staffordshire potteries.

There are huge masses or rather mountains of white granite in Cornwall, and in some parts of the county this is found to be partially decomposed; and it is where this is the case, that the mineral is raised and prepared for the purpose of making earthenware and porcelain, it having been discovered some years ago, by Mr Cookworthy of Plymouth, that this mineral furnishes us with the true *kaolin*, and also with the *petuntse* of the Chinese.

Granite is composed of quartz, fel-spar, and mica: and Mr Gerhard found, in making an experiment on granite, that the fel-spar melted into a transparent glass; under it the mica lay in the form of a black slag, and the quartz remained unaltered.*

To render the decomposing granite of Cornwall fit for the manufacturer, the following method is adopted. The stone is broken up by means of a pickaxe, and then thrown into a stream of running water. This washes off the light argillaceous parts and keeps them in suspension, whilst the quartz and the mica, thus separated, are allowed to subside near the place where the stone is first raised.

At the end of these rivulets are a certain kind of catch-pools, where the water is at last arrested, and time is allowed for the pure clay, with which the water is thus charged, to separate entirely from it. As soon as all the clay has subsided, the water is drawn out of these receptacles. The solid matter is then dug up in square blocks, and laid on what are called *linnees*, which are several connected series of strong shelves, erected so as to allow a proper circulation of air, in order that the clay may be more effectually dried for sale. It is then packed in casks and sent to the Staffordshire potteries, and elsewhere, under the name of China-clay. Thus prepared, it is extremely white, and in the state of an impalpable powder. It may here be observed that none of the native clays are entirely free from foreign ingredients. If pure argillaceous earth be required, it can only be procured from alum, which may be dissolved in water, and then decomposed by an alkali. In this process the pure clay will subside, which should then be thoroughly washed for use.

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* Nicholson's *Chemical Dictionary*.

The price of these Cornish clays is considerably higher than that of any other kind; but for many of the finer purposes of pottery, they are absolutely indispensable. They are very smooth and ductile, and their extreme whiteness is very remarkable. It was analysed by Mr Wedgwood, and found to consist of 60 parts alumina and 20 of silica.

The Cornish clay is also extremely useful for making crucibles, as it will not fuse by the addition of any of the common fluxes. In using these crucibles, however, they should always be put within others which are of little value; the inner ones will then acquire the heat gradually, otherwise they would be in great danger of breaking.

Besides the clays already mentioned, some of the Cornish granite itself is often used with the clay on account of the binding quality which it possesses, and its knitting as it were the other materials more closely by its fusibility, it being naturally more fusible than the earths usually employed in this manufactory: for it is a curious fact, that neither clay, silica, nor lime, will melt singly; but by mixing these three species in due proportions the greatest degree of fusibility is acquired.*

Having thus briefly described the several articles which are most commonly employed in making pottery, it will now be necessary to explain in what manner these are separately prepared for use.

The flints are managed in this way: they are first burnt in a kiln constructed for the purpose, and somewhat similar in form to a common lime-kiln; and then they are ground between very hard stones by a great power, which is given either by the water-wheel or by a steam-engine. In constructing the apparatus the purity and hardness of the stones are of very material consequence; for should they contain calcareous earth, a portion of this would be abraded during the grinding of the flint, which would prove very detrimental to the pottery of which it formed a part.

To enable the operator to expedite the process, and at the same time to grind the flints finer, a quantity of water is thrown into the mill with them, so that the article, when finished, has much the appearance and the consistence of cream. The health of the workmen is also preserved by this expedient; for, before this method was adopted, the atmosphere of the room often became charged with the finer particles of the flint, which, entering into the lungs, sometimes produced the most disastrous consequences.

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* More on this subject may be seen, under the article *Clay*, in Macquer's *Additions to the French Chemical Dictionary*.

The grinding the flint with water was first practised by the celebrated Brindley, well known for his skill in the construction of inland canals. The mills now in use were also invented by him; and are composed of a very hard siliceous stone called chert, which is found in abundance in the neighbourhood of Bakewell in Derbyshire.

I have already hinted, that it is important to attend to the nature of the stone which is employed in these mills, and some years ago I was told of a very severe loss which was sustained by many of the large potters, in consequence of their having been supplied with great quantities of prepared flint which had been ground by stones containing a considerable portion of carbonate of lime, which being mixed with the flint, and eventually with the material itself, occasioned to the body of manufacturers a loss of many thousand pounds.

In order to prepare the clay, it is first mixed with water to the consistence of cream: the design of this is, that it may be the more easily passed through the sieves, and intimately united with the ground flint, which could not be done with uniformity and certainty if these earths were mixed dry.

The mixture of the clay and water in the first instance, is generally effected by means of long wooden instruments, which the men move backwards and forwards with considerable force throughout the whole mass. It is an operation of great labour, and is distinguished by the term *blunging*.

The ground flint is mixed in a similar way with water, and the stirring is continued until it become one uniform fluid mass. But these earths, the clay and the flint, are always blunged *separately*; because, if otherwise, and the two earths were mixed before being deprived of their impurities, which cannot be done till they are brought into a fluid state, it would be impossible to observe the requisite proportions of each.

The Purification of these earths is effected by a process similar to that which the chemist calls Elutriation. When the earth has been well broken and thoroughly mixed with water, it is put into vats, or other large vessels; and when it has stood long enough for the grosser parts to subside, the fluid containing the finer particles is drawn off by a plug, and reserved for use.

In every preparation of these materials for the formation of earthenware or porcelain, it is of the utmost consequence to attend to the relative quantities of each: therefore, whenever these mixtures of the fluid clay with the flint are to be made, they are always done with some nicety, an attention being paid to the specific gravity of each of the fluids, which is either made heavier, or diluted further with water, as the case may require. The mixture of clay and water after it has passed *once* through the sieve, is generally brought to the specific gravity of 24 oz. the wine pint. A pint measure of the ground flint and water, when in the same state, is usually 32 ounces.

When the clay and the flint are mixed in suitable proportions, the whole mass, in this state of semifluidity, is passed through the sieves to take out any remaining impurities, and also to detain those particles which have not been sufficiently levigated. The mixture is then passed through the finest silk lawn, to ensure the certainty of no unbroken pieces remaining in it, and to reduce the whole to a state of the utmost uniformity and smoothness.*

The next object is, to separate the water which has been employed with such profusion, and the readiest way of effecting this is found to be by the direct application of heat. The mass is therefore now poured into very long brick troughs, which are built with flues under them of a sufficient size to afford heat enough to produce an ebulition in the mixture; and this is continued until so much of the water is evaporated as will reduce the mass to the desired consistence.

The evaporation is, indeed, never carried beyond a certain point; because, if this mixture of the earths were allowed to become nearly dry, it would not be kneadable by the usual methods; neither could it be worked, by the hand or the wheel, into any of those forms which it may be the intention or the interest of the potter to impart to it.

I ought to have mentioned, that the mixture of the clay and the flint with water, is known in the trade by the name of *slip*; that the place in which it is kept is the *slip-house*; and the trough on which it is boiled is called the *slip-kiln*.

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* A curious account of the Spanish method of preparing the clay for making the alcarazas, or wine-coolers, may be seen in a Memoir by Lasteyrie, in the *Journal des Mines*, for 1798.

When the mixture has been thus brought to a proper consistence, it ought to be kept for a considerable time, that the materials may become more intimately united than they can ever be by mere mechanical force: but in general this is not much attended to, although every manufacturer is fully aware of its importance. Want of room, of time, and often of capital, occasions it to be sometimes used warm from the slip-kiln, and not uncommonly when it has been lying for only a few hours or days. In China it is no unusual thing for the prepared clay to remain from 14 to 20 years before it is thought to be in a fit state for use; and I have heard of some districts where it is customary for a father to prepare as much porcelain-clay as will be sufficient for the use of his son throughout the whole period of his life.

When the dried mass has been removed from the kiln, the next process is that of tempering it, which is effected by first beating it with mallets, and then turning it and beating it again, and again, with small spades, or paddles, as the workmen call them, until it is thought to be as well tempered as it can be by these operations.

It should be recollected that this is not pure clay, but a mixture of all the ingredients. Vauquelin says that "Silex constantly forms at least two-thirds of most pottery; alumine from a fifth to one-third; lime from one five-hundredth to one twenty-hundredth part; and iron from the minutest quantity to twelve or fifteen per cent."

The mass, after the operation of beating, undergoes a process called *slapping*. For this purpose it is removed, in the state of large lumps, to a convenient bench or table, where a man having cut it across with a brass wire, unites it again by slapping one of the halves down upon the other with all his force; he then cross-cuts it again and again, and as often unites it by main force, as before.

This cutting the clay in pieces and slapping it together, is a laborious operation; but it must always be continued until the air-bubbles, which the mass at first contained in great abundance, are all driven out of it; because, if the air was not thus carefully expelled from the clay, it would escape when it became rarified in the oven, and this would produce such blisters as would spoil the manufactured goods. On this account the slapping is continued until the mass exhibits, wherever it may be cut, a perfectly smooth and homogeneous surface.

Of late years Mr Wedgwood and some other considerable potters have, however, employed an apparatus invented by Mr Thomas Lowe of Nottingham, which effects all these purposes at once. It is moved by a steam-engine, and performs all these operations with a surprising economy of time and labour. It first breaks the burnt flint; it then grinds it in water, and another part of the apparatus performs the hitherto laborious operation of blunging* the clay in water and breaking it into a semi-fluid and uniform mass. When this is done, it is run off and sifted by several sieves of different degrees of fineness, all which are moved by the same power. This earthy fluid then runs down to a lower room, where it goes through other sieves in the same way. In like manner, another part of the apparatus squeezes the dried clay, to save the expensive operation of *slapping*, described at page 455; after which it passes to an iron cylinder full of knives, where it is cut into pieces with great expedition; which saves the expense of cutting it by hand with brass wires, as before mentioned. This receptacle is not properly a cylinder, but a large hollow inverted cone of iron, and the knives are fixed in an upright shaft which revolves within it and passes through its centre. These knives are so contrived that the clay, as it is cut, is forced lower and lower by every revolution, until at last it is pressed by the power of the machinery through a square hole near the bottom of the apparatus, from whence it is removed as it falls, and is carried in blocks to the other parts of the manufactory.

The clay having undergone those varied operations, it is now fit for being formed into any shape, or employed for any purpose, for which it is designed. Accordingly, it is fashioned into various forms by means of a variety of moulds made of plaster of Paris; or put into as great a diversity of shapes by the potter's wheel.

Nothing is found to answer so well for making the potter's moulds as plaster of Paris; because, when rightly prepared, it has the property of absorbing the water so very rapidly from the ware, that it occasions it to slip out of the mould, or to *deliver* itself easily, as the technical phrase is.

To prepare the plaster for making the moulds, it is first ground in a mill exactly similar to that which is generally used for grinding corn by means of a pair of flour stones; it is then baked, or rather boiled,* to expel the water which is one of its component parts when in a native state.

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* This is the technical term, but doubtless it is a corruption of the word *plunging*.

This plaster, which is the native sulphate of lime, is boiled in very long brick troughs, having a fire flue running underneath them. The man who superintends the process has a handkerchief tied over his mouth for safety; as the small particles getting upon the lungs, or into the stomach, have been found to be very injurious to the health of the operator.

When this plaster has been thus deprived of water, it is in the state of a soft and impalpable powder; and if its own proportion of water be again mixed with it, it will immediately set into a hard compact mass. It is this property which renders it so peculiarly fit for making the potter's moulds.

The manipulations by which the different articles of earthenware are made, are so various and multiplied, that it would be very difficult, if not impossible, to describe them. They can be known only by those who have had opportunities of visiting a pottery. A short outline of these processes will however be expected from me, and may be acceptable to the generality of my readers.

Much of the common earthenware is formed by means of the potter's wheel, which is a round board attached to a lathe, and capable of being moved thereby, either rapidly or otherwise, as the occasion may require. This round board moves in a horizontal position; and when in use, the clay which is to be fashioned is fixed on the centre of it; and it is put in motion either by a person who constantly attends it when at work, or by means of a treadle which is moved by the foot of the workman himself.

As the clay revolves upon this machine, the workman either models it by his fingers, or forms it, by means of an instrument which he holds in his hand, into any kind of circular shape that he may desire; and when the object is to make a number of vessels exactly similar to each other, the size is generally determined by a guage fixed without the circumference of the revolving wheel, but projecting over it in such a manner that, whenever the yielding clay is spread out until it touch this guage, the artist knows that the article which he is making has attained the exact figure which he intends.

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* *Boiled.* This may appear to be an incongruous expression when applied to a dry earthy substance; but the fact is, that when the ground plaster is heated it has so much the appearance of ebulition, that the term which I have employed is made use of by the workmen universally. The finished article is always called *boiled plaster*.

The potter's wheel is probably the most ancient of the manufacturing instruments now in use. It was known to the author of that book in the Jewish Scriptures, which claims the highest antiquity of any writings we have, and who must have lived at least 3000 years ago. This simple instrument has however of late years been much improved by adapting a strap to it, which passes over a large taper cylinder of wood, by means of which the artist is enabled to increase or diminish the rapidity of the motion at pleasure. This contrivance is known to mechanics by the name of the *cone pulley*.

In going through a pottery, it will be observed, that all the articles are very thick and coarse when they come from the wheel. This perhaps is unavoidable. They are therefore taken to a lathe, where the extra quantity of clay is removed, in the same manner as the wood-turner forms his work by shaving off successive portions of the wood. These parings of clay are afterwards kneaded as before, and then worked over again for similar purposes.

It is curious to notice the great variety of circular things that may thus be made by means of the potter's wheel; but it must be remembered that this machine is calculated only for cylindrical, conical, or round articles, and not for such as are of an oval or an irregular form. All such are made in plaster moulds, which divide in halves for the convenience of taking out the ware whenever it is found to be sufficiently dry to be removed. The handles of cups and jugs are also made in moulds, and these are afterwards put on the vessels for which they were designed.

The consumption of sulphate of lime, gypsum, or alabaster, (for this substance is known by these various names,) is so considerable for making the moulds for plates and dishes, that in the course of the year many tons of worn out moulds are thrown away as useless. I would suggest, that these might be ground and used as manure, instead of purchasing fresh gypsum for that purpose.

As the plaster moulds absorb water with avidity, the articles which are made in them dry much sooner than would generally be imagined. For instance, the moulds on which the common table-plates are formed, are put into a very temperate stove as the plates are made, and in about two hours the whole will be dry enough to be removed to make room for a fresh parcel. In the usual way of conducting a manufactory, every mould is capable of taking four or five fresh plates in the course of a day of twelve hours. The plate-stove is generally a very small room built with bricks, and shelved with boards from the floor to the ceiling. It is usually heated by an iron pipe which passes through it.

When the plates are removed from this stove, they are taken off the moulds, and pared round the edges with a small knife; and when they have been slightly polished by the hand, they are laid by to become hard enough for the biscuit oven.

From this short account it may be easily conceived that a common-sized pottery must require a great deal of room, especially as every article, whether made by the wheel, the lathe, or the moulds, must be spread out upon shelves to dry: and this reminds me of an expedient which I lately observed at the works of an ingenious potter in Staffordshire, and which I conceive to be so far an improvement upon the old practice, as to deserve being mentioned, and which I have the leave of the proprietor to make public.

The improvement to which I refer, is that of having a chamber shelved entirely round, not with stones or boards as is usual, but with shelves of prepared plaster made as smooth as polished marble, and of the thickness of an inch and a half each.

Upon these plaster shelves, instead of those of wood and stone as commonly adopted at other works, the goods are placed to dry: and the great advantage which must accrue to the manufacturer from the practice is very obvious; for the plaster not only absorbs the water sooner, but the articles will dry more regularly and uniformly than they possibly can when placed upon substances which cannot abstract the water. Being persuaded of the utility of this expedient, I have no hesitation in recommending it for general imitation. Moreover, why may not the floors of a potter's warehouses be covered with plaster? The expense would be very inconsiderable, and then, whenever these were not in use for storing the finished ware, they might be advantageously employed in hastening the drying of the goods newly made.

In some counties it is usual to form the floors of cheese chambers with plaster; and I have been told that the cheese becomes sooner ripe on these floors than on others; though Mr Twamley directs that it should not be placed on plaster until time has been allowed for the first moisture to exude and evaporate.*

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* Josiah Twamley *On Dairying*, 8vo. Warwick, 1784, p. 100.

An important improvement might be made in the manufacture of earthenware, if it were possible to contrive an expeditious mode of spreading the goods out so as they might always dry gradually. The property which clay has of contracting by heat or by drying is well known, and the excellence of earthenware depends in a great measure on its being very *gradually* dried before it be exposed to the kiln.

When the articles of pottery have been modelled or fashioned to the design of the artist by any of the foregoing methods, and have been partially dried, either by an exposure to the action of the atmosphere, or by placing them for a certain time in stoves or drying rooms constructed for the purpose, they are carried to the potter's oven, where they are placed in deep oval boxes made of fire clay. These are called *seggars*; and being flat at bottom, one of these vessels forms a cover for another, so that the workmen are enabled without difficulty to place them in piles nearly to the top of the oven, which is a large building of brick, in the form of a cone, and very similar to a common glass-house.

When the oven is properly filled, heat is applied by means of ignited coal, which is thrown into several receptacles built on the outside, but communicating, by means of flues, with the inner part. These are called *mouths*; they stand about four feet in height from the ground, and project about three feet from the sides of the kiln.

In the beginning the heat is kept at a low state; but after a lapse of twelve hours it is gradually increased, and the augmentation of temperature is continued until the kiln and its contents acquire the proper maximum.

An attention to the degree of baking is an object of extreme importance. Vauquelin has remarked that "the heat should be such as to expel the moisture, and agglutinate the parts which enter into the composition of the paste; but incapable of effecting the fusion, which, if too far advanced, will render the ware of so homogeneous a texture as to become brittle."*

It has been found that alumina loses 0.46 of its weight even when the fire is not urged beyond the temperature of ignition for silver; whence it appears that it contains nearly half its weight of water, and it does not part with the last portion but with great difficulty.

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* Vauquelin's *Reflections on the Qualities of Pottery*, communicated to the Philomathic Society at Paris.

The operation of burning usually lasts two days and two nights. This, however, varies in the different charges; and the fireman judges when the ware is sufficiently burnt, by examining the trial pieces which are placed in those parts of the oven where they may be taken out with ease, and without causing any diminution of the heat.

These trial pieces are made of the common red clay, of the Staffordshire brick-makers. It is found in the Potteries, and has the property of changing its colour at every change of temperature: therefore, by comparing one of these, taken out of each part of the oven, with a trial piece made of the same kind of clay and properly burnt, and which is kept on purpose for a standard, the exact state of the ware under operation may at any time be exactly known.

(*To be continued.*)

ART. XLIV.—*Account of the Discoveries and Experiments of the Swedish Chemists during the year* 1825. [*Brewster's Journal.*]

1. *Professor Berzelius's Discovery of Lithia in Mineral Waters.*

PROFESSOR BERZELIUS has been occupied with the examination of several mineral waters from Bohemia, viz. those of the Eger, or Franzensbad, and those of Marienbad. These waters were found to contain the same substances which this chemist detected in those of Carlsbad, the analysis of which has been for some time before the public,* but in the new analysis he has found also *lithia*. The quantity of the carbonate of the alkali is very small, particularly in the waters of Carlsbad, and in that of Eger; but the waters of the spring called Kreuzbrunn, at Marienbad, contain as much as a centigramme of the carbonate of lithia in every bottle.

The following is M. Berzelius's method of discovering this alkali in any solution. He precipitates the lime by means of oxalate of potash, and afterwards separates the magnesia by carbonate of soda, but the mixture must be evaporated to dryness, and the residue fused; for otherwise some of the magnesia would be easily redissolved in the form of a double carbonate of soda and magnesia. The mass taken up by the water and filtered, will not give any farther precipitation even when pure phosphate of soda is added; but if it contains lithia, it will become turbid during the evaporation, which must be continued till the matter be perfectly dry. It is next redissolved in a very small quantity of cold water, which leaves undissolved a double phosphate of soda and lithia, equivalent to one-third of its weight of carbonate of lithia. The characters which distinguish this phosphate from the earthy phosphates with which it may be confounded, are as follows: it is very fusible before the blow-pipe. When melted with carbonate of soda, it enters with the soda into the charcoal. On a leaf of platina the melted mixture is limpid. The earthy phosphates remain on the charcoal while the soda penetrates it, and do not give a limpid mixture when they are melted on a leaf of platina. With twice its weight of carbonate of lime, it fuses at a red heat, without, however, attacking the platina, as lithia ordinarily does; but if some drops of water are added to it, and afterwards evoporated, the platina becomes yellow all round when the mass is heated anew.

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* See *Edin. Journ. of Science*, vol. ii. p. 176, January, 1825.

2. *Professor Berzelius's Experiments on the Orange Gas produced from a Mixture of Fluor-Spar and Chromate of Lead.*

As the English and French Journals have already given an account of Professor Berzelius's experiments on the different combinations of the fluoric acid which have facilitated the reduction of *Silicium*, *Zirconium*, and *Tantalum*, we shall not at present enter upon the subject.

A German chemist, M. Unverdorben, has published some experiments on the fluoric acid, the most curious of which was that in which, after mixing together fluor-spar and chromate of lead, he distilled them in a leaden retort, with fuming or anhydrous sulphuric acid. From this there resulted a gas, which could not be collected, because it destroyed the glass. This gas gave a very thick yellow or red smoke. It was readily absorbed in water, which was then found to contain a mixture of chromic and flouric acids. When it came in contact with air, the gas deposited small red crystals, which were those of chromic acid.

Professor Berzelius repeated these experiments of M. Unverdorben, and he found that the experiment succeeded equally well with common concentrated sulphuric acid. He collected the gas in glass flasks covered with melted resin, and filled with mercury. The gas had a red colour. It gradually attacks the resin, deposits chromic acid in its mass, and penetrates even to the glass, which it decomposes without change of volume, the chrome being replaced by silicium. Ammoniacal gas introduced into it burns with explosion. Water dissolves it, and yields an orange-coloured fluid, which, evaporated to dryness in a platina dish, leaves as a residue pure chromic acid. The fluoric acid volatilizes entirely. This method is at present the only one which gives chromic acid perfectly pure.

If the gas is received in a platina vessel of some depth, whose sides have been slightly wetted, and into the bottom of which the gas has been made to descend, the water begins to absorb the gas, but, bye and bye, crystals of a fine red colour are seen to form themselves round the opening of the metallic tube which conveys the gas, and in a short time, the vessel is filled with a red snow, consisting of crystals of chromic acid. The fluoric acid dissipates itself in vapour, and absorbs entirely the water added at the beginning of the experiment. These crystals have this curious property, that, when they are heated to redness in a platina dish, they begin at first to melt, and afterwards, by a slight explosion, accompanied with a flash of light, they decompose themselves into oxygen gas, and the green protoxide of chrome. The chromic acid which has been dissolved in the water does not present this phenomenon. It fuses during its decomposition, but it does not give a flash of light. This difference does not arise from its containing water, for it is perfectly free from it when it is heated to a little above 100° centigrade.

M. Unverdorben had already observed, that crystals of chromic acid, introduced into ammoniacal gas, are decomposed with a flash of light. The ammonia is destroyed, and the acid leaves the protoxide as a residue. It is necessary to make these experiments quickly, as the crystalized acid is deliquescent.

In distilling chromate of lead with chloride of sodium, we obtain a gas similar to the preceding, and which contains chrome combined with chlorine, in such proportions, that the water, by its decomposition, gives rise to the formation of the hydrochloric and chromic acids. The gas is red, and may be collected over mercury, but it is very much charged with chlorine, when it is prepared by means of the common concentrated sulphuric acid, whose water of combination destroys a certain quantity of the gas.

3. *Account of Professor Berzelius's Method of detecting Arsenic in the Bodies of Persons poisoned.*

Professor Berzelius has lately given some instructions for the discovery of arsenic in persons that have been poisoned with it. He considers the *reduction of arsenic to the metallic state as the only incontestible proof of the presence of this poison.* Arsenic may occur in two ways, viz. when it is found in substance (in the state of arsenious acid) in the dead body, and when it is not found in this state; though the intestines of the dead body may contain it in the state of a solution.

In the first of these cases, it is easy to determine the presence of arsenic. In order to do this, take a piece about three inches long of an ordinary barometer tube, and having drawn out one end of it, into a much narrower tube, close the narrow end. Let some of the arsenic found in the body be now put in at the open end, so that it may fall down to the closed end. Any quantity of this arsenic of sufficient volume to be taken from the body will suffice for this purpose. The arsenic being at the end of the tube, a little charcoal is let fall upon it, after it has been freed from all moisture by bringing it to a red heat with the blow-pipe. The charcoal is then heated in the tube at the flame of a spirit-lamp, the arsenic being held out of the flame. When the charcoal is very red, the point containing the arsenic is drawn into the flame. The arsenic is then instantly volatilized, and passing into vapour by the red charcoal, it is reduced, and reappears on the other side of the flame in a metallic state. The flame is then brought slowly towards the metallic sublimate, which is thus concentrated into a smaller space in the small tube, and then presents a small metallic ring shining like polished steel.* We have now only to verify, by its smell, that the metallic sublimate is arsenic. For this purpose, cut the small tube with a file a little above the sublimate, and having heated the place where it lies, put the nose above it at a small distance, and the particular odour of the metal will be immediately perceived.

In the case where the solid arsenic cannot be found, we must collect as much as possible of the contents of the stomach and the intestines, or even cut the stomach in pieces, and mix it with its contents. The whole is then to be digested with a solution of hydrate of potash. Hydrochloric acid is then added in excess. The whole is filtered, and, if the liquid is too much diluted, it is concentrated by evaporation. A current of sulphuretted hydrogen is then passed through it, which precipitates the arsenic in the form of the yellow sulphuret. If the qnantity of arsenic is very small, the liquid will become yellow without giving a precipitate. It must then be evaporated, and, in proportion as the hydrochloric acid becomes more concentrated, the sulphuret of arsenic will begin to be deposited. It is then filtered. If the sulphuret remaining on the filter is in two small a quantity to be taken from the paper, add some drops of caustic ammonia, which will dissolve it. Then put the liquid which passes the filter into a watch-glass, and evaporate it. The ammonia will be volatilized, and will leave as a residue the sulphuret of arsenic. If it shall still be difficult to collect the sulphuret, we must put into the watch-glass a little pulverized nitrate of potash, and, with the finger, mix the sulphuret with the nitrate of potash, which detaches it from the glass. At the bottom of a small phial, or a piece of glass tube, shut at one end, melt a little nitrate of potash at the flame of a spirit-lamp, and introduce into it, when melted, a little of the mixture which contains the sulphuret of arsenic. It is oxidized with effervescence, but without fire, or detonation, and without loss of arsenic. The melted salt is then to be dissolved in water, and lime added in excess, and the liquid boiled. The arseniate of lime will then be deposited, and may be collected. When dried, it is mixed with charcoal, and then brought to a red heat by the blow-pipe, and a small quantity of this mixture is allowed to fall to the small end of the above tube. It is now gradually heated to expel all humidity which ends to throw it into the wide tube, and when it is very dry, heat at the flame of the blow-pipe, the part of the tube which contains the mixture. The arsenic will be disengaged, and be sublimed, at a distance from the heated part. An addition of vitrified boracic acid greatly promotes the decomposition which then takes place at a less elevated temperature; but this acid frequently contains water, and produces a bubbling of the melted matter which raises it in the tube, and causes the vapours to issue by perforating the softened part of the glass.

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* Had the experiment been made in the wide part of the tube, the result would scarcely have been visible with a small quantity of arsenic.

M. Berzelius maintains that the *sixth part of a grain of sulphuret of arsenic is sufficient to make three different trials;* but he adds, that, when we have discovered only very small traces of arsenic, we must take care not to introduce any by means of re-agents, among which, both the sulphuric and the hydrochloric acid may contain it. The first almost always contains some arsenic when it is not manufactured from volcanic sulphur, and the second, in consequence of sulphuric acid being used in the preparation of the hydrochloric acid, yields the arsenic which it contains in separating it from soda. We must, therefore, be certain of the purity of these re-agents.

When death has been caused by the arsenic, and not by the arsenious acid, the process must be modified, because the sulphuretted hydrogen gas decomposes the arsenic acid too slowly. In this case, we must add hydrosulphuret of ammonia, which reduces the arsenic acid to the state of sulphuret, which is afterwards precipitated by the hydrochloric acid.*

4. *Professor Berzelius's Researches on Molybdana.*

In studying the properties of molybdæna, M. Berzelius has found that this metal, of which we knew only the purple oxide, produced by drying the blue oxide, and molybdic acid, has two salifiable oxides, whose saline combinations were till now unknown. The deutoxide may be procured by digesting a mixture of molybdic acid, metallic molybdæna, and sulphuric or hydrochloric acid, till the colour of the liquid becomes a deep red. Instead of metallic molybdæna, we may substitute metallic copper. The red liquid gives with ammonia, a rust-yellow precipitate, which is the hydrate of the deutoxide of molybdæna. This hydrate is very soluble in water. When it is washed, the water, after having removed the saline substances, which caused its precipitation, begins to dissolve the hydrate, and becomes yellow. It at last dissolves it entirely, and the saturated solution is red. It reddens turnsol. The hydrate dissolves in acids, and gives salts, whose solutions are red, but which, when evaporated to dryness, are almost black.

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* It is obvious that Berzelius has not seen Dr Christison's paper on the "Detection of minute quantities of arsenic in mixed fluids." These gentlemen agree in precipitating arsenious acid by sulphuretted hydrogen, so as to obtain the yellow sulphuret; but their subsequent methods differ: Berzelius adopts a process which requires all the dexterity of as expert a chemist as himself for conducting it with success. Dr Christison, on the contrary, scrapes the sulphuret from the filtre with a knife, which may be done though a very minute portion of it is present, and obtains metallic arsenic at once by heating it with black flux. We refer for particulars to his paper in the *Edinburgh Medical and Surgical Journal.*

The protoxide is produced when we macerate the solution of a salt, with a base of the deutoxide, with mercury, and add, from time to time, a liquid amalgam of potassium. The colour of the liquid becomes deeper, and ends by growing black. Before the introduction of the amalgam, we must add to it hydrochloric acid, in order to prevent a part of the deutoxide from being precipitated before its entire reduction to the protoxide. The black solution is then precipitated by ammonia, and the black precipitate is the hydrate of the protoxide, which must be well washed, and then dried *in vacuo*. The hydrate appears then under the form of a jet black powder. When heated *in vacuo*, it gives out slowly its water, and afterwards, at a temperature which approaches to that of brown-red, it takes fire, and burns with scintillation. The barometer of the air-pump is not affected by this phenomenon, which, in other respects, is of the same nature as that which is observed in the hydrate of the peroxide of iron, and the protoxide of chrome, and of zircon. The anhydrous protoxide is insoluble in acids. When heated in air, it takes fire, and burns feebly, producing the brown oxide of molybdæna. The salts of this oxide are black, and their dilute solutions have a compound colour of green, black, and brown, though sometimes they assume a fine purple colour. The fluate of the protoxide, for example, is a very fine purple, and the double fluates with potash, soda, and ammonia, are of a rose red colour.

In order to form the protoxide of molybdæna, we may make use of zinc in place of the amalgam of potassium, but the protoxide then retains the oxide of zinc in a very obstinate manner.

What is called molybdous acid, that is to say, the blue oxide of molybdæna, is not a particular acid. It cannot be combined with alkalis, which, on the contrary, decompose it, by precipitating the hydrate of the yellow oxide, and combining with the molybdic acid. It may be produced most readily in dissolving the bimolybdate of ammonia, and adding to it a solution of a salt with a base of the deutoxide. It produces a precipitate of a fine deep blue, which is very soluble in water, and is only deposited because the water contains salts. We may wash it with a solution of sal ammoniac, afterwards removing the salt by a little cold water. It gives with warm water a blue solution, highly saturated, which may be easily preserved at the ordinary temperature of the atmosphere. In the dry form it resembles indigo, and retains its solubility in water.

Professor Berzelius has found, that the deutoxide of molybdæna is composed of one atom of molybdæna, and two atoms of oxygen. The molybdic acid contains three atoms. The blue oxide is a bimolybdate of the deutoxide of molybdæna, that is $\ddot{M}o + \dddot{M}o$. There is still another combination between the oxide and the acid which is produced when the blue liquid is digested with metallic molybdæna. It is green, equally soluble in water, and precipitable in sal ammoniac. M. Berzelius supposes its composition to be $\ddot{M}o + 2\,\dddot{M}o$. Tungstic acid likewise combines with the deutoxide of molybdæna, and the combination is very soluble in water, and of a superb purple colour. It is also precipitated by sal ammoniac.

The molybdic acid performs the part of a base towards the stronger acids. M. Berzelius has examined them in this point of view, and has described some of the salts which it forms.

M. Berzelius has discovered a new sulphuret of molybdæna, proportional to the molybdic acid. It is of a ruby colour, transparent and crystallized. It combines with the metallic protosulphurets, and forms with them particular salts, of which a great number are soluble in water.

Molybdæna combines with chlorine in three proportions. The first is red, and a little volatile. The second is black, very fusible, very volatile, and crystallizes in a black mass, of a brilliant colour, like iodine, which it resembles even in the colour of its gas, which, however, is more red than violet. The third is colourless, and crystallizes in scales. These three chlorides correspond to the muriates of the protoxide, of the deutoxide, and of the peroxide, that is to say, of the acid. Iodine does not combine in the dry way with molybdæna, but the hydriodic acid dissolves the protoxide and the deutoxide. The molybdic acid decomposes it, and separates the iodine from it.

The best method of obtaining molybdæna, in some quantity, is to heat the molybdic acid in a porcelain tube. When this tube is heated to redness, there is introduced into it a current of hydrogen gas, which is continued as long as it produces water.

5. *M. Setterberg's Experiments on the Sulphurets of Cobalt.*

M. Setterberg has found that the deutoxide of cobalt decomposes sulphuretted hydrogen gas when cold. The sulphuret thus produced contains three atoms of sulphur.

Hydrochloric acid dissolves some of the cobalt in it, and leaves another sulphuret of cobalt in the form of a black powder, which contains *four* atoms of sulphur for *one* atom of metal.

6. *M. Mosander on the Precipitation of Magnesia by Carbonate of Soda.*

During the analysis of a new species of noble serpentine, M. Mosander made an observation which merits the attention of chemists who are occupied with the analysis of minerals. He remarked, that when the carbonate of soda is used to precipitate magnesia, the precipitate contains a double carbonate of soda and magnesia; that the alkali cannot be removed from it by edulcoration with water, and that the washings always contain magnesia. It is necessary, therefore, to evaporate the alkaline liquid to dryness, and melt the salt to render the magnesia caustic. As carbonate of potash is commonly used for this operation, there is no risk of being exposed to this inconvenience; but it is evident, that whenever the liquid contains a salt with a base of soda, even though potash is used to precipitate the magnesia, the double carbonate of soda and magnesia ought to be precipitated.

7. *M. Mosander's Analysis of the Oxides of Iron, formed by continued Heat.*

It is well known that M. Berthier analyzed the cinder which is formed on pieces of iron intended for being laminated to form iron-plate, and that he considered it as a new degree of oxidation of iron, which contained 1¼ as much oxygen as the protoxide. M. Berzelius kept a piece of iron intended for iron-plate, forty-eight hours in a furnace. The oxidated crust was two lines thick, and exhibited, what M. Berthier has also observed, two distinct layers. M. Mosander undertook the analysis and examination of it. It was at first evident, that M. Berthier had analyzed together two distinct substances. The interior layer is more black than the other, has a grained fracture of little lustre, and is very slightly magnetic. The exterior layer has a brilliant metallic lustre, a bright and shining fracture, and a grey metallic colour, and, what is very remarkable, has a powerful action on the magnetic needle.

M. Mosander found that the layers consisted of

	Inner Layer.	Atoms.	Outer Layer.
Peroxide of Iron,	27	1	36
Protoxide of Iron,	73	3	64

The formula for the first is $\dddot{Fe} + 3\,\dot{Fe}$. The analysis of the outer layer is the same as M. Berthier had obtained for the two layers mixed together. This approached to $\dddot{Fe} + 2\,\dot{Fe}$.

But, as on this occasion, the oxidated layer was supplied with oxygen from the outer layer, and with iron from the interior layer, it may be supposed that there was an insensible gradation from the most oxidated to the least oxidated side.

M. Mosander was thus led to analyze different zones of the two layers; and he found that at the exterior layer this was actually the arrangement. The external surface consisted of the red oxide, quite pure; below this was the ordinary magnetic oxide (*oxidum ferrosoferricum* of Berzelius), and farther in the quantity of protoxide gradually increased to the commencement of the interior layer, which he found homogeneous throughout. It appears, then, to be determined by these experiments, that we have two combinations of the peroxide of iron with the protoxide, that is, the magnetic oxide $\dot{Fe} + \dddot{Fe}$, and the less magnetic one which forms the inner layer of the oxidated crusts produced upon metallic iron by heat, $\dddot{Fe} + 3\,\dot{Fe}$.

Art. XLV.—*The Geological Deluge, as interpreted by Baron Cuvier and Professor Buckland, inconsistent with the Testimony of Moses and the Phenomena of Nature.* By the Rev. John Fleming, D. D., F. R. S. E.—*Edin. Phil. Jour.*]

The science of Geology was first introduced to public notice, in this country, by philosophers who, while they cherished a reverential regard for the authority of the Scriptures, overlooked those methods of investigation which lead to a discovery of the laws of nature. Assuming that the first principles of geology were revealed to Moses, and communicated in the Book of Genesis, they were satisfied with a comparison of the scanty notices there given of the history of the earth with the phenomena presented by its surface, even when the character and relation of these phenomena remained in a state of comparative obscurity. The original condition of the materials with which the Creator formed this globe, long occupied the attention of those early cosmogonists; and, as the history of Moses was too meagre in its details to serve their purpose, and the earth failed to exhibit the suitable documents, the imagination was called upon to supply that which neither the words nor the works of the Deity furnished. These reveries, however, usually termed Theories of the Earth, do not call for any comment at present.

The cause by which the deluge was produced, and the changes which it affected on the appearance of the globe, occupied the second place in the estimation of these geologists. Here, again, the details of Revelation were so deficient as to lead some to suppose that our copy of the Book of Genesis was more abridged than the one possessed by the ancient Jews.—(Kirwan's *Geol. Es.* 48.) The surface of the Earth was hastily looked at for proofs of the effects of this catastrophe; and again imagination supplied that which observation could not yield. Burnet brought the waters from below, through the broken *crust* with which he fancied they had been covered during the antèdiluvian period, and with the fragments of this crust he formed the mountains. Woodward suspended, for a time, all *cohesion* among the particles of earth, and reduced the globe to a soft paste; while Whiston, not inferior in fancy to any of his predecessors, called a *comet* to his aid.

While philosophers were thus claiming the attention of the public in favour of their efforts to roconcile geology with revelation, they were powerfully assisted by individuals of another description. The "Place of Descent" where the "Ark" rested, had long been regarded as determined; remains of the timber had been preserved; and many pieces of the bitumen, with which it was caulked, had been carried off to be employed as amulets for averting mischief. The skeletons of the antediluvian inhabitants were eagerly sought after; and the Continent of Europe seemed to furnish the expected documents: even the grinders and thigh-bones of the antediluvian giants were disinterred from those graves which for so many ages they had occupied.

As science advanced, these theories of the deluge appeared in their true light; as unsupported by the statements in Scripture, and as inconsistent with the phenomena of nature. The skeleton of the antediluvian man became that of an acknowledged reptile; while the grinders and thigh-bones of the giants were admitted to belong to elephants. The geologist beheld his theories vanish like a dream, and the admirer of revelation felt (though very unnecessarily) as if a pillar of his faith had become a broken reed. Geology, by these premature attempts at generalization, fell into discredit as a science among philosophers, and by the christian it was viewed with suspicion. The former had witnessed opinions and assertions substituted for facts; and the latter had reaped the fruits of misplaced confidence. The friend of revelation had begun to consider the history of the deluge as the least perfect of those records which Moses has transmitted, since no proofs could be found in nature to attest the occurrence of the catastrophe. Need we be surprised, therefore, that a considerable degree of anxiety should prevail on this subject, with the religious public, and that any fresh attempt to revive their hopes would meet with a cordial, I had almost said a credulous, welcome? The truth of this view has been put to the test.

Baron Cuvier, so deservedly celebrated as a comparative anatomist, having devoted much labour to the investigation of fossil bones, naturally directed some portion of his attention to those collateral subjects which might serve to illustrate their history. In the preliminary discourse to his great work on "Fossil Bones," he announced the important results to which his labours, reading, and reflection had conducted him. This discourse was published in Edinburgh, in 1813, under the title of "Cuvier's Theory of the Earth." It has gone through several editions, and still continues to be a favourite with the public. It has contributed, in a very great degree, to render the study of geology popular in this country. How far the explanations which it offers of the phenomena of nature are true, and how far they are consistent with the sacred writings, will afterwards be considered.

The Reverend William Buckland, while Professor of Mineralogy in Oxford, appears to have embraced Baron Cuvier's views respecting the deluge; and, under their influence, distributed the modern strata (exclusive of the volcanic) into Postdiluvian detritus and Diluvian detritus.—(*Phil. Geo. England and Wales*, 1818.) In his "Inaugural Lecture," which was delivered May 15, 1819, before the University of Oxford, on the endowment of a readership in geology, he selected for the title, "Vindiciæ Geologicæ; or the Connexion of Geology with Revelation explained:" and stated in the dedication, "that the facts developed by it (geology) are consistent with the accounts of the creation and deluge recorded in the Mosaic writings." In his subsequent inquiries, this learned and indefatigable Professor, who has contributed so much to exalt the geological character of England, has not only investigated the history of those beds of gravel and clay which contain fossil bones, but has successfully explored many caves which he considers as having been the *dens* of antediluvian animals. The results of these inquiries he has published in his "Reliquiæ Diluvianæ," in which he considers geology as "attesting the action of an universal deluge." This work, like the "Theory" of Cuvier, has greatly contributed to render the science of geology popular, by bringing it into favour with the Church, and even securing the countenance of the drawing-room. The general reader has been charmed with the novel scenes which it discloses, while the christian has hailed it with joy, as offering a valuable testimonial to the authority of revelation.

To my "Remarks illustrative of the influence of Society on the Distribution of British Animals," inserted in No. XXII. of this Journal, Professor Buckland has conceived it necessary to make a "Reply," which has a place in No. XXIV. In this communication, he continues to advocate the opinions which he had advanced in the "Reliquiæ Diluvianæ," and attempts to obviate some of the objections which had been, incidentally, stated against them.

In an inquiry of this kind, regarded as highly interesting to the philosopher and the christian, it seems requisite to exercise extreme caution. The fate of former theories in geology, which professed to explain the phenomena of nature, and to strengthen the authority of revelation, but which inquiry demonstrated to be visionary, intimates the risk of error, and calls for a minute examination of the value of the proofs adduced. I enter upon this inquiry as one deeply interested in the authority of revelation, and not indifferent to the progress of geological science. My remarks may not appear convincing, but they may excite that inquiry and discussion which lead to truth. It is impossible, however, in a paper of this kind, to enter into all the details which the general reader would probably desire. The outlines only of the subject can be noticed.

In reference to this important subject, two questions naturally occur to the mind: 1. Does the character of any of the members of the modern strata demonstrate the occurrence of a universal flood, as exclusively the agent in their formasimilar opinion with less hesitation: "The circumstances of their character clearly evince, that they also have escaped from the last grand catastrophe, perhaps by another route than the races of the Caucasian and Altaic chains, from whom perhaps, they may have been long separated before the epoch of that catastrophe." On the supposition that the different races of men were derived from a common stock, an idea sanctioned by revelation, supported by the truths of zoology, and tacitly admitted by our author, it seems difficult to discover any proof of their separation having been antediluvian. According to Moses, all that escaped of the human race, were eight individuals of the family of Noah. Here, then, we have the character of the geological deluge, in reference to the human race, as interpreted by Baron Cuvier, standing opposed to the history of the deluge as given by Moses, and that, too, in its most important feature.

2. The geological deluge, as interpreted by Baron Cuvier and Professor Buckland, occasioned the destruction of all the individuals of many species of quadrupeds. As examples of those which have thus suffered extinction, may be quoted, the fossil elephant, fossil hippopotamus, fossil rhinoceros, fossil bear, and fossil hyæna, besides many others. These have been, somewhat presumptuously, termed ANTEDILUVIAN ANIMALS.[*] In the history of the Noachian deluge, as given by

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[*] In my first paper, in No. xxii. of this Journal, I have stated that the relics of these ancient animals occur in postdiluvian strata. The learned Professor in his "Reply," first declares, "That, could the above cases be established, they would be decisive in favour of the theory maintained by Dr Fleming;" and shortly after adds, that, "Even admitting all those facts, still every atom of the evidence contained in my Reliquiæ Diluvianæ would remain unaffected by the discovery," I attempt not to reconcile such apparent contradictions. Perhaps it may be judged reasonable to allow an adversary, when hard pushed, to shift his position, even though it put the pursuer to more trouble. With reference to the Rhinoceros horn from Forfar, about which Professor Buckland is unnecessarily prolix, I may state, that I relied on the authority of Professor Jameson, in the Wern. Mem. iv p. 582; and having seen the horn labelled, as from Forfar, in the Edinburgh Museum, of of which he is Regius Keeper, I still consider the statement of Professor Jameson to be substantially true, and the one given by my opponent as quite the reverse. The bottom of the horn attests its origin,—the numerous rents and their marly contents. The Blair-Drummond example I quoted from the same authority. It is singular, that, in the same number of the Journal in which this case is likewise treated as spurious, and in the Proceedings of the Wernerian Society, I found "Notices regarding the Rhinoceros Horns of Blair-Drummond, tending to shew that they may probably be regarded as having occurred in the blue clay of that district; by Mr A. B. Blackadder, Allan Park," p. 401. As Professor Buckland has admitted, in his "Reply," my first example of *extinct* animals being *postdiluvian*, I have got quite

Moses, it is expressly stated, that clean and unclean beasts, fowls after their kind, cattle after their kind, and every creeping thing of the earth, two of every sort, male and female, were taken into the ark, preserved in the ark, and brought forth in safety from the ark, and dismissed with the mandate of their Creator to breed abundantly on the earth, and to be fruitful and multiply upon the earth. Here, then, we have revelation, declaring that, of *all species* of quadrupeds a male and female were spared and preserved during the deluge; while we have the phenomena of nature, as interpreted by the geologists we have quoted, intimating that *all the individuals, of many species*, were not spared, not preserved, but *annihilated*, by the catastrophe. An error must exist in one of these statements. The declaration of Moses is positive. The phenomena of nature may not have been suitably investigated. Shall we reject then, the conclusions of the geologist, and respect the authority of Moses, or give the preference to Cuvier and Buckland?

3. According to Baron Cuvier, "this revolution had buried all the other countries which were before inhabited by men, and by the other animals that are now best known; and the same revolution had laid dry the bed of the last ocean, which now forms all the countries at present inhabited." (Theory, p. 171.) Moses expressly tells us, that the flood of waters was upon the earth, prevailing exceedingly upon the earth, and covering the highest hills; that the waters returned from off the earth. Here, again, we have the opinion of Cuvier, in direct opposition to the whole tenor of the history of the Noachian deluge. Nor need we be surprised at this, since he seems to be in opposition to himself. At one time he supposes, that the innundation did not reach to the summits of the higher mountain chains; and that Mongols, Caucasians, and Negroes may have escaped by different sides, or by different routes; at another, that the bed of the antediluvian ocean is now the abode of the post-diluvian quadrupeds.

4. The geological deluge, as interpreted by Professor Buckland, was sudden, transient, universal, simultaneous, rushing with an overwhelming impetuosity, infinitely more powerful than the most violent waterspouts. In the history of the Noachian deluge by Moses, there is not a term employ-

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enough to establish my views. The acknowledged postdiluvian character of the gigantic elk is as decisive as any horn of a rhinoceros in a marl bed, or carcase of a mammoth in a postdiluvian iceberg.

ed which indicates any one of the characters, except universality, attributed to the geological deluge. On the contrary, the flood neither approached nor retired suddenly. The waters rose upon the earth, during the continuance of the rain, for forty days; and they retired slowly, upon the rain being restrained. There is no notice taken of the furious movements of the waters, which must have driven the ark violently to and fro. On the contrary there is reason to believe from the writings of Moses that the ark had not drifted far from the spot where it was at first lifted up, and that it grounded at no great distance from the same spot.

5. The geological deluge, as interpreted by Professor Buckland, excavated, in its fury, deep valleys, tearing up portions of the solid rock, and transporting to a distance the wreck which it had produced. On this supposition, the aspect of the antediluvian world must have been widely different from the present; lakes, and valleys, and seas, now existing in places formerly occupied by rocks, and the courses of rivers greatly altered. In the Book of Genesis there is no such change hinted at. On the contrary, the countries and rivers which existed before the flood do not appear, from any thing said in the Scriptures, to have experienced any change in consequence of that event. But if the supposed impetuous torrent excavated valleys, and transported masses of rocks to a distance from their original repositories, then must the soil have been swept from off the earth, to the destruction of the vegetable tribes. Moses does not record such an occurrence. On the contrary, in his history of the dove and the olive-leaf plucked off, he furnishes a proof that the flood was not so violent in its motions as to disturb the soil, nor to overturn the trees which it supported; nor was the ground rendered by the catastrophe unfit for the cultivation of the vine.

Viewing, in connexion, these differences between the Mosaic history and these interpreters of the phenomena of nature, it seems impossible to admit, that, "as far as it goes, the Mosaic account is in perfect harmony with the discoveries of modern science." The reverse appears rather to be the case. It is well known, that Linnæus declared that he saw no examples in nature of the ravages of a universal flood: "*Cataclysmi* universalis certa rudera ego nondum attigi, quousque penetravi; minus etiam veram *terram Adamiticam*; sed ubique vidi factas ex æquore terras, et in his mera *rudera* longinque sensim præterlapsi ævi," (Syst. Nat. iii. 5.;) and this opinion has given offence to several well disposed friends of revelation, who have, nevertheless, formed their notions of the deluge from the speculations of geologists, instead of the records of Scripture. I confess that I entertain the same opinion as Linnæus on this subject; nor do I feel, though a clergyman, the slightest reason to conceal my sentiments, though they are opposed to the prejudices which a false philosophy has generated in the public mind. I have formed my notions of the Noachian deluge, not from Ovid, but from the Bible. There the simple narrative of Moses permits me to believe, that the waters rose upon the earth by degrees, and returned by degrees; that means were employed by the Author of the calamity to preserve pairs of the land animals; that the flood exhibited no violent impetuosity, neither displacing the soil, nor the vegetable tribes which it supported, nor rendering the ground unfit for the cultivation of the vine. With this conviction in my mind, I am not prepared to witness *in nature* any remaining *marks* of the catastrophe, and I feel my respect for the authority of revelation heightened, when I see on the present surface no memorials of the event. On the other hand, had I witnessed every valley and gravel-bed, nay, every fossil bone, attesting the ravages of the dreadful scene, I would have been puzzled to account for the unexpected difficulties; and might have been induced to question the accuracy of Moses as an historian, or the claims of the Book of Genesis to occupy its present place in the sacred record. Instead of finding the Deity setting his bow in the cloud, as a pledge that he would not again visit the earth with a flood, and as the *only natural token* of what had happened; I had expected to find a reference made to every diluvian heap of gravel, and every valley of denudation, as a memorial of that wrath which was displayed, while visiting rebellion with death. In other words, if the geological creeds of Baron Cuvier and Professor Buckland be established, as true in science, then must the Book of Genesis be blotted out of the records of inspiration. But as I believe in the authority of the Mosaic history, and see, in the opinion of Linnæus, a strict conformity therewith, in letter and spirit, I may perhaps be asked, How can I reconcile the phenomena of nature, as interpreted by these geologists, with the view which I have embraced? I have already, in my first paper, declared, that "the works and the words of God must give consistent indications of his government, provided they be interpreted truly. It has been announced, that the Mosaic account is in *perfect harmony* with the discoveries of modern science, though we have pointed out a *palpable disagreement.* Perhaps a similar difference may exist between these supposed discoveries of modern science and the phenomena of nature. Our attention will now be directed to the determination of this important point, involved in the second question we proposed to discuss. As now to be examined, it is one exclusively of a scientific character, in which all our appeals must be made to the facts established by observation or experiment.

II. *Does the Character of any of the Members of the "Modern Strata," demonstrate the Occurrence of a Universal Flood as, exclusively, the Agent in their Formation?*

The progress of truth, in this branch of the inquiry, must necessarily be correlative with our knowledge of the "modern strata," and the causes which have operated in their production. Whether a sufficient degree of knowledge has been acquired, or sufficient attention been bestowed on the subject by British geologists, I leave to the determination of the unprejudiced. Enough, in my opinion, seems to have been secured to enable us to solve the question under consideration.

Various conjectures have been offered by different geologists, respecting the origin of the waters of the deluge. Some are disposed to consider the waters of the earth as sufficient, if once set in furious motion. A few look to a sudden change in the Earth's axis as the origin of the catastrophe, in the absence of all proof from the science of Astronomy. Some consider the waters as having been set in motion by the attractive force of a comet, without previously gaining an affirmative answer to the question; has a comet this attractive force? There is abundant proof that the planets disturb the comets, but the converse is not known. The comet of 1454 eclipsed the Moon; while that of 1770 not only came near the Earth, but passed through the midst of the satellites of Jupiter, without producing any sensible effects. Others, translating the phrase of Moses, "the windows of Heaven," as literally meaning "a comet's tail," have considered the water as added to the Earth. I would be disposed, before admitting this view of the matter, to ask, Is the *vapour* of a comet's tail *aqueous?*—The following phenomena, however, bear more directly upon the question under discussion.

1. *Excavation of Valleys.*—Valleys, in the opinion of the supporters of the diluvian hypothesis, may have been pro-

duced by different causes, such as irregularity of deposition, or subsequent dislocations of the strata. But those which exist in rocks nearly horizontal, "must be referred exclusively to the removal of the substance that once filled them; and the cause of that removal appears to have been a violent and transient inundation." Valleys of this kind have been designated by the very inappropriate term, "Valleys of Denudation," as if they had been only *exposed*, not *formed*, by the catastrophe. Many circumstances seem to oppose the diluvian hypothesis, in reference to the origin of valleys; among which, the following may be noticed.

a. *Shape of Valleys.*—The valleys of denudation are not always straight in their course; they have their salient and re-entering angles, their lateral branches, and their increase in width as they descend. When we look at a valley, at present forming, by the action of running water, in beds of clay or gravel, we witness the sinuosities of its banks produced by the oscillations of the stream *at the bottom*, now transporting the materials from one side, then from another, and thus aiding the force of gravity in causing the loose matter of the bank to descend. The lateral branches are produced by a similar process; and the valley widens as it advances, by the increase of its waters from the lateral streams, and the consequent increased transporting power. I am in the habit of employing an old-fashioned logic, and comparing small things with great, referring analogous phenomena to the same cause, and proceeding from the distinct to the obscure. Under the influence of these principles, I feel myself compelled to conclude, that the old valleys, with the characters described, have been produced, like those forming under my eye, *by the long-continued action of running water at the bottom.* How a sudden, transient, and universal flood, covering the highest hills, could have produced these effects, I cannot conceive. The main branch must have been first scooped out; then the subordinate lateral branches, in succession; and a current in the main branch following each, to clear away the rubbish. Had the lateral currents been flowing simultaneously with the principal one, a *bar* would have been formed at the mouth of each branch; and if there had been no *succession* of currents in the main trunk, it would have been filled with the materials of the lateral branches. To those who have studied the natural history of rivers, especially their junctions with other rivers or with friths, the force of the objection will be obvious.

It has been objected to the theory of the excavation of valleys by running water, that *now* no water flows through them. But water *may* have flowed through them, though now absent. The bursting of a lake, at a higher level, may have cut off the sources of several springs, and directed water through a distinct and very different channel from that in which it formerly flowed.

b. *The Impotence of Water as an Abrading Power.*—The advocates of the diluvian hypothesis, have, in their zeal, committed that mistake intimated by the schoolmen, "Causam assignare quæ causa non est." It is impossible to form an adequate conception of all the effects which might result from a violent and transient inundation, covering the highest hills, and sweeping whole continents with destructive fury. The mind is lost in the vastness of the operation, and the imagination is left, unfettered, to pursue its reveries,—a most bewitching predicament for a geologist. But we may make an approach to the subject. When a river is in a violently flooded state, we witness it remove the soil which opposes its current, transport to a lower level the *loose blocks* of rock, and sweep away the animal and vegetable productions occurring in its course. But it is subject to certain limitations. Throughout its course, its velocity is greatest at the surface and the middle of the stream, from which it diminishes toward the bottom and the sides, where it is least. When it enters a hollow, lake, or mill-pond, the water below the outlet has its motion checked, and, in its state of comparative stillness, permits the heavier materials it had transported to subside. When a water-spout descends almost in a solid column of great height, and exerting, consequently, a pressure well calculated to remove obstructions, it penetrates the soil, and disperses it, along with the vegetable covering, removes the loose blocks of stone, and the surrounding detritus, while it makes but a feeble impression on the solid rock. When an alpine lake bursts its barriers, it acts precisely as a river in a flooded state; carries along with it soil, loose rocks, trees and animals, depositing at a lower level the wrecks of its course,—as happened in the Val de Bagnes, by the bursting of the lake of Mauvoisin, (*Edin. Phil. Journ.* No. I. p. 190.)

Let us now suppose a body of water (no matter at present whether fresh or salt), of sufficient height to cover the highest mountains, and possessing a progressive motion of great velocity, suddenly to arrive at the north of Zetland, traverse the kingdom, and pass off towards the south, at the Land's End, What would be the accompanying phenomena? The soil would be every where annihilated in its progress, and, as mud, transported to a distance. The animal and vegetable inhabitants would be floated off. All detritus, boulders, and loose blocks of rocks, would, at the onset, yield to its pressure and velocity. But every lake, every valley, every lee side of a hill, every frith and bay of the sea, would speedily be in a state of comparative stillness, and receive the largest and the heaviest of the transported blocks. In the bottom of valleys and lakes we should now find the wreck of the catastrophe. But, have we the shadow of evidence to warrant the conclusion, that this inundation could tear up solid rocks, and make excavations in undisintegrated strata? No. The force of cohesion, or rather crystallization, is more than a match for water falling from any conceivable height, or moving with any known velocity. The numerous islands which occur around our coasts, even where most exposed, and the cascades so common in the hilly districts, attest the absence of this abrading or excavating power. Did it possess this power, the Straits of Dover and the Pentland Frith must by this time have become unfathomable; Niagara should have ceased as one of the wonders of the world, and wooded valleys should have occupied the place of the Canadian lakes.

While I deny to water this *abrading* power, because the whole history of rivers is in opposition, I willingly admit its *transporting* power after disintegration has taken place,—a distinction to which the student in geology would do well to take heed.

c. *The Terraces in Valleys.*—In many valleys, on the Continent of Lurope, in this country, and in America, terraces occur in the banks, which, from their horizontality, indicate their production by water at the period these valleys were lakes. Several terraces may be traced in some valleys, and these, according to Professor Buckland, "shew the number of successive stages by which the bursting of the gorge took place."—*Rel. Dil.* 217.) In Lochaber four such terraces occur, shewing four successive eruptions. These terraces, however, are declared to be "all of postdiluvian origin."—*Ib.* Whatever be the era of these terraces, they demonstrate a few truths, which cannot be very agreeable to the supporters of the diluvian hypothesis. Many lakes formerly existed, where valleys now occur; and there are agents in Nature capable, at different intervals, of opening the barriers of these lakes, and permitting the water to escape suddenly. Such lakes and such agents may have existed before the flood. Each bursting must have resembled a deluge in its effect upon the district through which the waters passed, and the wrecks which it would accumulate at the lower level. When, therefore, we witness a valley, the present waters of which empty themselves by a narrow gorge, how are we to determine whether that gorge has been opened before the deluge, at the deluge, or after the deluge? The Vale of Pickering, in Yorkshire, may be taken as an example. According to Professor Buckland, it was an antediluvian lake (it would have been, from its characters, a valley of denudation, had it not been necessary to have a sheet of fresh water for the antediluvian hippopotami to swim in;) the deluge opened the gorge at Malton, and converted it into a postdiluvian valley. But it is just as probable that it was a postdiluvian lake, and that the gorge of Malton was removed by an agent, similar to that which opened its northern neighbours in Lochaber. When we see a valley, the waters of which flow out at a gorge, we may infer that it was formerly a lake. We may also infer that a sudden deluge could not tear away the barrier rocks, unless previously disintegrated; and we may watch the transporting power of the present stream: but if we have any geological caution, we will hesitate about fixing the era of the change.

These terraces are found in greater numbers in alpine districts, as might have been anticipated. They occur, however, even at low levels. I have already noticed three examples in this Journal, and I have more to produce. They are much more numerous than is commonly imagined. Even in the valley of the Thames there is reason to believe they exist, though this hollow is pronounced, by Professor Buckland, a valley of denudation.* Mr Greenough, a strenuous supporter of the diluvian hypothesis, has stated in his Geology (p. 121), that "the valley of the Thames, in London, is contained in that of which Clapham Rise forms part of the boundary on one side, and the Green Park on the other; and this, again, is contained in the larger valley, which occupies the interval between Highgate and Sydenham. Arrived at these points, we find our horizon bounded by a chalk ridge still loftier." These included valleys throw great light on the history of the globe. They are like the circular valleys in river courses: they mark some of the features of a former state of things; they assist us in tracing the changes which have taken place, and even the agents concerned in their production: but they give us no dates.

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* In the "Reply"† I am accused of supporting one of my conclusions "by stating, on the *misinterpreted* authority of Mr Trimmer's paper," that several of the reputed antediluvian animals occur in the postdiluvian, regularly stratified clay, &c. But how is this grave charge of misinterpretation supported? "I venture (he says) to assert, that no remains of this kind have ever been found in the peat bogs of any part of the valley of the Thames, still less in the regular stratified clay, *that is*, the *London clay*." Had I really said that

† See Edin. Jour. vol. ii. p. 549.

(To be continued.)

Mr Trimmer found these remains in the "London clay," the charge would have been well founded, as he says that they occur *above* the *London clay*. But I say no such thing. Is the London clay (in the geological sense of the term) the only *regular stratified clay* with which my opponent is acquainted? This cannot be the case. Or can he deny, that the "Brentford clay," is less regularly stratified than the "London clay?" I use the phrase, *obviously* consistent with the authority which I quote; and I was the more inclined to do so, for the purpose of exhibiting the distinction between this *regularly stratified* clay and the ordinary diluvium, which is *irregular* in its structure. So far, therefore, *I* have been misinterpreted, not Mr Trimmer. But there is still a difference between us. Professor Buckland says, that he has visited the clay in question, and pronounces it diluvium. Last spring, when in London, I was anxious to see a genuine example of *diluvium*, and the more so, as Mr Trimmer's remarks indicated a very different deposition: and because I had suspected that the advocates of the diluvian hypothesis were in the habit of confounding together, at least, two of the "modern strata." Nor was I disappointed; for that which my opponent has pronounced diluvium, I found to be *Lacustrine Silt;* and my conclusion rested on the following facts: 1. The beds, and their strata of fine clay and sand, are nearly horizontal. 2. They contain, here and there, thin horizontal patches of small rounded flinty pebbles, (precisely similar to small layers of gravel which I had seen in genuine examples of similar origin), indicating the influence of occasional floods. 3. Scattered through the clay I observed several pieces of shells, the present inhabitants of our lakes or slow running streams, viz. *Helix peregra* and *complanata*, *Turbo fontinalis*, and *Cardium corneum* of Montagu. It is evident, therefore, that a lake existed here which has been filled up by slow degrees, and the character of the materials, and organic remains of the different beds, mark certain epochs in the process. It is fortunate that this example occurs so near London as to be of easy access to the members of the Geological Society. Perhaps a good deal of the reputed English diluvium may, upon investigation, be found to be lacustrine silt, as in the present instance.

ART. XLVI.—*Remarks on the Geology of the Vicinity of Boston.*

By J. W. WEBSTER, M. D.

[Continued from vol: II. p: 292.]

AT the close of the remarks on the geological structure of the country in this vicinity, in a former number of this Journal, it was stated that a remarkable feature in the conglomerate at Roxbury, was the occurrence of numerous fissures and rents in every part of the beds. In a few places these rents have obviously been produced by trap rocks; in some of them the trap is seen in thin veins partially decomposed, while in others it remains firm and unaltered. At the north-eastern extremity of this bed of conglomerate, where the rock has been extensively quarried, a larger portion of what was originally a vein of great size, has been left projecting from the conglomerate, standing like a ruinous wall. This vein or dyke is composed of imperfect prismatic masses placed at right angles to the direction of the vein, and these are slowly crumbling to powder, from the joint operation of those atmospheric agents whose influence is so great and beneficial in resolving the hardest and most sterile rocks into a fruitful soil. In the immediate vicinity of this vein are several smaller veins which have undergone no apparent change, and which are seen to begin and end in the contiguous conglomerate. Several of these smaller veins, varying in width from three inches to the tenth of an inch, can be seen in close contact with the conglomerate, and even the thinnest of them are divided by cross rifts into the prismatic masses so characteristic of these rocks. At the junction of the conglomerate and trap no change appears to have taken place in the texture of the conglomerate or of any of the imbedded nodules. The general course of these trap veins is from 10° W. of S. to 10° E. of N. they contain minute particles of iron pyrites, which, by the action of the air and moisture, impart to the rock a ferruginous aspect, and cause it to separate in concentric crusts. The principal vein is found on examination to contain detached portions of the conglomerate.

At the distance of about 25 yards in a westerly direction from the large vein, is another of smaller dimensions, being but 6 inches wide, which has an inclination towards the east; it cannot be traced to any considerable distance, being lost in the accumulated fragments at the bottom of the quarry.

A short distance beyond this last, another vein is seen to begin and terminate in the conglomerate, being a good example of what would be considered by many geologists as a contemporaneous formation. Notwithstanding the occurrence of these trap veins, it is a circumstance worthy of notice that no rounded portions of the same rock are to be discovered as constituent parts of the conglomerate itself, a fact which goes to show that the two rocks are of different ages.

Pursuing the direction of the conglomerate from Roxbury through Brookline towards the west it is seen to become finer grained, and at the same time to acquire more of a slaty character; becoming in places distinct grau wacké. The whole aspect of the rock now begins to change, and at the entrance of the town of Brighton it becomes a well characterised amygdaloid. In a quarry about half a mile S. W. of the meetinghouse in Brighton the passage of the two rocks into each other is finely displayed. A vertical section of this quarry shows the conglomerate occupying the lowest part, upon which is a thin bed of a slaty character, approaching to amygdaloid in its structure, while the perfect amygdaloid reposes upon it, a stratum of loose pebbles and small boulders being interposed on each side of the slate. It is difficult to ascertain the relative position of the amygdaloid and conglomerate, except in one or two places where denuded, and from these it would appear that the former is the superincumbent rock. In several places, however, within the limits of the town of Brighton the transition from the one rock to the other may be satisfactorily observed.

The amygdaloid contains veins of quartz, sometimes well crystallized epidote, sulphates of baryta and lime, carbonate of lime, chlorite, micaceous iron, sulphuret of copper, and crystals of adularia; near the Agricultural Hall in Brighton it forms steep precipices, and contains a somewhat singular variety of quartz in very thin sheets. The quartz is often coloured green by chlorite and becomes a beautiful prase. Attempts have been made to saw this amygdaloid into slabs and to polish it for ornamental purposes, but, as might have been anticipated, the process of cutting and polishing laid open cavities which frustrated all attempts to obtain a uniform surface.

The character of the amygdaloid of Brighton is different from that in which agate nodules are met with in Europe, and it is probably to be referred to an earlier epoch. In general it is harder and more compact than what was formerly considered by the disciples of Werner as a Flœtz amygdaloid, and although it cannot be advantageously used in ornamental architecture, it is a good and durable building stone. This rock covers most of the hills in the town of Brighton, being more compact in some than in others, and not unfrequently acquiring a coarse slaty structure. It may be traced into the adjoining towns, and at Newton Falls it becomes of a brown colour and passes into the conglomerate.

Returning to the south and southwestern side of Boston, we find the conglomerate occupying an extent of several miles. It is the prevailing rock in the town of Dorchester, and appears here and there in the towns of Milton, Quincy, and Canton, being bounded to the south and east by the extensive formation of sienite, whence so abundant a supply of one of our best building stones is obtained.

Road to Milton.—Passing over the great deposite of clay at South Boston, we cross the conglomerate of Dorchester, and observe it acquiring a greater degree of compactness and uniformity of composition until within about 3 miles of the Blue Hills, where it passes into compact felspar, and this last into hornstone. Soon after commencing the ascent of Milton Hills an outgoing of sienite is to be seen, which is probably connected with that of Quincy. This sienite is covered by a peculiar porphyry resembling some of the trachytes, and composes the principal part of the Blue Hills.* It forms steep precipices, the escarpments being towards the S. W. The porphyry of the Blue Hills contains a large proportion of quartz, and the felspar has a reddish colour; towards the bottom the felspar predominates.

On the southeastern side of Boston the conglomerate may be traced about 6 miles towards the town of Quincy, where it is succeeded by a clay slate resembling that of the western part of Charlestown. The slate, after disappearing under the waters of Boston harbour, again emerges to form some of the islands with which it is studded. From the manner in which this slate is connected with the rocks that have been mentioned, there is reason to infer that it underlays all of them. The clay slate of Quincy is succeeded by sienite, which, along the shores of Hingham and Cohasset, is traversed by veins of trap approaching to basalt. The islands, which lie between these shores and the promontory of Nahant on the opposite side of the harbour are evidently parts of the same formation, and the slate of that promontory has undergone a striking change from the presence of the huge veins of trap with which it is traversed in every direction. The geological structure of the opposite shores and islands, presents many indications to the eye of the geologist, that the numerous islands in the harbour of Boston, are the remains of a once continuous formation. To the North, the same character continues; and in the sienite of Marblehead and the vicinity, we have the same geological character as in that of Cohasset.

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* The highest of the Blue Hills is about 710 feet above the level of the water in Boston harbour.

It has been seen that on the south, the transition rocks of our neighbourhood are overlaid by porphyry at Milton; the same is the case on the north, at Malden, where the hills are composed of a porphyry of various shades of red, green, and grey, equal in beauty to any which was worked by the ancients. This porphyry is however not the same as that at Milton; its base being a distinct hornstone, in which the crystals of felspar are not always readily discovered by the naked eye, as they often approach in colour to that of the basis itself. In the red and purple varieties, however, the felspar is beautifully distinct. It is a hard and durable rock, and at some future day it is to be hoped will be made to contribute to the permanence and magnificence of our public works. This stone is susceptible of the most perfect polish, and is every way similar to that which was wrought in former ages. That the art of working it is not lost is evinced by the beautiful vases and slabs which are occasionally brought to this place from the North of Europe.

Associated with the porphyry of Malden, is a bright red jasper, which forms a bed of considerable extent on the south western side of Saugus river.

In several parts of the extensive porphyry formation of Malden, thin veins of micaceous iron are met with, one of which was worked to some extent many years since, under the belief that it was a vein of lead. In the lowlands, and along the margins of the small streams in the neighbourhood, bog iron ore occurs in considerable quantity.

(To be continued.)

ART. XLVII.—*Answer to Professor Strong's "Remarks on Mr. Colburn's Theory of Parallel lines."* By WARREN COLBURN, Esq.

SINCE publishing my theory of parallel lines, I have observed in the notes of a late edition of Playfair's Euclid, a definition of parallel lines very similar to mine. This definition was used by Wolfius, by Boscovich and by Thomas Simpson, in his first edition of his elements. It is of course no new thing, but I had never seen it. It was not in the former editions of Playfair. The same objection is made to this definition in Playfair, that Professor Strong has made to mine. My demonstration founded on this definition is very different from that given in Playfair.

The objection is plausible at first thought; but on examination I think it will be found to consist in the peculiar mode of presenting the idea to the mind. This is no uncommon source of difficulty.

Euclid's definition of parallel lines is as follows:—"Parallel straight lines are such as are in the same plane, and which being produced ever so far both ways do not meet."

In this definition it is taken for granted that two straight lines can be so situated.

My definition is, "Two straight lines which are equidistant throughout their whole extent are called parallel lines."

In this of course it is taken for granted that two straight lines can be so situated; and I would ask the reader if this is not as easily granted without proof or explanation, as that of Euclid? I think it is.

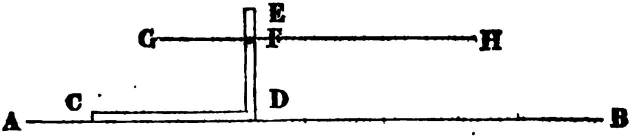
Euclid was farther obliged to take for granted the disputed proposition which I have attempted to prove; however, if my definition is as difficult to be granted, as Euclid's difinition and axiom together, of course I have effected nothing.

If my definition is granted, it necessarily follows that all straight lines, drawn from one of them perpendicular to the other, are equal, since it is easy to prove that the perpendicular to a straight line drawn from any point without the line, is the shortest distance, and of course the proper measure, from that point to the line.

But we will present Professor Strong's idea in a little different form, and see if it is as formidable an objection, as at first sight it appears to be.

Instead of the abstract form I shall give a mechanical description which will not take any from its generality. This mode is often employed by mathematician's in defining curved lines, as circles, ellipses, parabolas, &c.

Let AB be a straight line, and let a common joiner's square be supposed to be applied to this line, so that one edge of one leg CD may coincide with it, and the other leg DE will of course be perpendicular to the line.



Let a point be fixed somewhere at F in the leg DE. Now if we suppose this square to slide along upon the line AB, the point F will describe the line GH, all the points of which will be equidistant from AB, because FD is always perpendicular to AB. The question is, what kind of a line will GH be, straight or doubtful?

If Professor Strong's first objection is removed, as I think it must be in the minds of most readers, the second must necessarily follow it. For it must be allowed that the line AB (see fig. in the demonstration alluded to by Professor Strong) is as far from CD as CD is from AB. Now a perpendicular drawn from AB to CD measures the distance of the two lines asunder, also a perpendicular from CD to AB measures the distance, and the two lines were by hypothesis equidistant throughout their whole extent, therefore the abovementioned perpendiculars are equal.

General Intelligence.

New Work on Mineralogy.—Dr E. Emmons, has published a "Manual of Mineralogy for the Use of Schools, &c." which appears to be well adapted to the purpose for which it was intended. The design of the author was to furnish a text book for that excellent institution, the Rensselaer School, which should answer the purposes of introducing beginners to the study of minerals, and be a convenient book of reference for persons, who were travelling. Within the compass of about

200 pages, Dr Emmons has given the prominent external characters, and chemical composition of all the minerals, made known up to the time of publication. He appears to have consulted the latest and best works on the science, and has made an abridgment of them, well suited to the use of all schools, where the elements of Mineralogy are taught.

Andalusite of Westford, Massachusetts.—In the tenth volume of the American Journal of Science and Arts, at page 384, the occurrence of andalusite, at Westford, in this state, was noticed by Professor Hitchcock of Amherst College, who remarks that he picked up a few specimens from a stone wall, near the bottom of the hill east of the village, on the road leading to Chelmsford. Having lately visited the town of Westford for the purpose of obtaining specimens of this substance, I have had the good fortune to find it in considerable abundance.

The village of Westford is situated on a hill, composed chiefly of mica slate, dipping to the NE., under an angle of about 45°. The mica slate is well characterized, and towards Chelmsford passes into gneiss, both rocks being traversed by veins of granite and quartz. The mica slate is in several places much iron-shot, and the gneiss is here and there porphyritic. Numerous boulders of granite are met with, which increase as we approach Chelmsford.

The andalusite is found only in detached masses of quartz and mica slate; all my attempts to discover it *in situ* were unsuccessful.

The crystals of andalusite are of various shades of red, brown, and even black, varying in size from microscopic, to that of several inches in length, and an inch and a quarter on each face. Many of the crystals which I obtained present far more numerous replacements than have been noticed in any European specimens. The inclination of the primary faces is about 91° and 117°, of the secondary, about 132° 20′. Besides occuring in crystals, the mineral is met with massive, and intermixed with mica, often so intimately as to give the mass the appearance of emery.

The andalusite is accompanied with rhœtizite, and a fibrous quartz, resembling bucholzite. In a few specimens it is connected with graphite. Some of the crystals are almost perfectly transparent. The crystals resemble in external characters, and geological associations, those of the Sualpe in Carinthia and of Lahm in Bavaria; surpassing them however in size and transparency. JWW

Application of Caoutchouc to the Manufacture of Cordage, &c. A patent has been obtained by Mr Hancock of London, for an improvement in the manufacture of cordage and other articles from hemp, flax, and other fibrous substances. The improvement consists in covering the substances, when they are formed into yarns, strands, ropes, threads, &c., with liquid caoutchouc. In colour and consistence, the liquid resembles cream. It is applied to the cordage, &c. in a manner exactly similar to that employed by rope-makers in making tarred ropes, except that it is not heated.

From an analysis of the liquid caoutchouc, or original sap, as carried to England, it was found by Mr Faraday to consist of

(Solid) Caoutchouc	317,0
Albuminous precipitate	19,0
Bistre colouring matter, a highly azotated substance, (yielding a strong odour of ammonia)	71,3
Wax	
Substance soluble in water not in alcohol	29,0
Water	563,0
	999,3

On the changes in Mercurial Thermometers.—Mr H. H. Blackadder has proposed a new explanation of the cause of the change by which the accuracy of thermometers is affected. It had been supposed that the bulb of the instrument itself increased in capacity, an explanation which Mr Blackadder deems unsatisfactory. In the most carefully constructed thermometers some air is left within the instrument. It is supposed that in the course of years, this air will be decomposed by the mercury, the oxygen being absorbed; and in becoming solid, its bulk will be diminished. In this way the lowering of the mercurial column can be accounted for; in addition to which, Mr Blackadder supposes that a portion of air remains adhering to the inner surface of the bulb, which gradually escapes to the upper part of the tube. It is proposed to render the instrument more accurate, by allowing a considerable space of time to elapse between the construction of the glass part and the adaptation of its scale.

On the Air contained in River and Canal Waters.—Dr Ure has determined the proportion of air contained in these waters by boiling them.

	Grain Measures.	
18,000 grain measures of canal water (in winter) yielded	480 or	$\frac{1}{37.5}$
Filtered river water, drawn in the city of Glasgow from the pipes of the Cranston Hill Company	454	$\frac{1}{39.65}$
Filtered river water from the pipes of the Glasgow Water Company.	450	$\frac{1}{40}$
Water taken directly from the river Clyde, somewhat swollen by winter rains	505	$\frac{1}{35.64}$

The gaseous matter obtained from the first three waters, consisted of $\frac{1}{10}$ carbonic acid gas, and $\frac{9}{10}$ atmospheric air. That from the open river contained only $\frac{1}{20}$ of carbonic acid. The above waters, when submitted to examination, had a temperature of 55° Far.—*Brande's Journal.*

Preparation of Aluminum, by M. Oersted.—Of late, the attempt to produce the combination of chlorine with most substances has been successful; of the small number of compounds of chlorine which remained to be procured, was that with aluminum. The usual method not answering, it was necessary to attempt a new one. It consisted in passing a current of dry chlorine, through a mixture of pure alumina and carbon, equally dry, and placed in a porcelain tube heated to redness. There resulted carbonic acid, an excess of chlorine and of chloride of aluminum, the latter compound, being gaseous at high temperatures, is condensed in a receiver, and the chlorine and carbonic acid are disengaged and left in passing through a long tube which proceeds from the receiver. The chloride of aluminum is volatile, at a temperature not much above that of boiling water; it is slightly yellow, probably from a small excess of chlorine. It crystallizes, absorbs water with great avidity, and dissolves very easily with the disengagement of much heat. Warmed rapidly with an amalgam of potassium, a double decomposition takes place, and we obtain by distillation out of the contact of the air, a metallic mass, which in colour and lustre, approaches very much to tin, this is aluminum. For the rest, the author found some remarkable properties in the amalgam of aluminum, and in aluminum itself, which do not allow him to consider his experiments as finished, but which on the contrary promise important results.

We may obtain chloride of silicium by the same process; the latter is however more volatile than the chloride of aluminum. The two chlorides may be obtained by one operation, by passing a current of chlorine through an incandescent mixture of pure clay and carbon. The chloride of silicium obtained in this way, is so volatile that it requires artificial cold for its condensation, it is then liquid, colourless, and according to an experiment of M. Koster, its density is 1,5: it boils at 50 centigrade. Its elasticity is an obstacle to the formation of an amalgam of silicium, and when it is obtained the silicium may be extracted in the state of a brown metallic powder as described by Berzelius.—*Ferussac's Bulletin, April.*

Selenium from Lukawitz in Bohemia.—There is a considerable manufactory of sulphuric acid, belonging to Prince Auersperg, at Lukawitz, in the circle of Chrudim in Bohemia. The selenium, as at Gripsholm in Sweden, is contained in the brownish mud deposited in the lead chambers. According to the experiments of Professor Steinmann, of Prague, this mud contains about four per cent. of selenium. The ore employed for extracting the sulphur is common iron pyrites, imbedded in mica slate. Professor Steinmann, who already possesses upwards of six ounces of pure selenium, has contrived a method of concentration, by exposing a mixture of this sediment, or of the sulphur containing selenium, extracted from it by melting, and sulphuric acid, to a previous distillation. The greatest portion of the sulphur is oxidized and driven off in the shape of sulphurous acid, leaving a residue in which the content of selenium is predominant, to be treated afterwards, as usual, with nitro-muriatic acid. Sulphur, containing about twenty per cent. of selenium, of a more or less deep orange yellow, is sold at Prague, at the price of twelve shillings a pound. Trials have been made, though hitherto unsuccessful ones, to mould the selenium in basso-relievos, representing the portrait of Berzelius.—*Brewster's Jour.*

Account of the Process of MM. Thenard and Darcet for preserving Substances from Humidity.—On the 27th February 1824, there was read at the Academy of Sciences of Paris, a *Memoir* by MM. Thenard and Darcet, on the employment of fatty bodies for making coverings and unalterable plasters, and for making moist places salubrious. This process, the effects of which have been established by several years experience, consists in causing a mixture of one part of oil and two parts of resin to penetrate, by means of an intense heat, either porous stones or plaster. The bodies penetrated with this mixture acquire afterwards a singular degree of solidity, and become absolutely impermeable to moisture.

This process may be employed for rendering low and damp places salubrious. It was tried at the Sorbonne, and the expence of it was only 16 sous per square metre, or a square whose side is 39 English inches. The other objects to which it is proposed to apply it are houses, statutes placed in the open air, bas reliefs and sculptures in plaster, the ceilings and walls of rooms intended for Fresco paintings, basins for holding water, and reservoirs for holding grain.

M. Thenard exhibited to the Academy several objects of art executed in plaster by his process. In order to show its efficacy, he exposed to the open air for several years a bas relief, half of which was formed of ordinary plaster, whilst the other half was prepared. This last half was perfectly preserved, while the other displayed visible traces of disintegration. This process does not resemble those which consist in covering bodies with a sort of skin which keeps off humidity. The body is actually penetrated with the mixture to the depth sometimes of several inches.

Population.—In Great Britain, the number of individuals in a state to bear arms, from the age of 15 to 60, is 2,744,847. The number of marriages is about 98,030 yearly; and it has been remarked, that in 63 of these unions there were only 3 which had no issue. The number of deaths is about 332,708 yearly, which makes nearly 25,592 monthly, 6398 weekly, 914 daily, and 40 hourly. The deaths among the women are in proportion to those of the men as 50 to 54. The married women live longer than those who continue in celibacy.' In the country, the mean term of the number of children produced by each marriage is 4; in towns the proportion is 7 for every two marriages. The number of married women is to the general number of individnals of the sex as 1 to 3; and the number of married men, to that of all the individuals of the male sex, as 3 to 5. The number of widows is to that of widowers as 3 to 1; but the number of widows who marry again, is to that of widowers in the same case, as 7 to 4. The individuals who inhabit elevated situations live longer than those who reside in less elevated places. The half of the individuals die before attaining the age of 17 years. The number of twins is to that of ordinary births as 1 to 65. According to calculations founded upon the bills of mortality, one individual only in 3126 attains the age of 100 years. The number of births of the male sex is to that of the female sex as 96 to 95.—*Edin. Jour.*

New Method of preparing Quills.—The following is the manner in which M. Schloz of Vienna proceeds in the preparation of quills for writing by means of which he renders them more durable, and even superior to the best Hamburgh quills. For this purpose he makes use of a kettle, into which he pours common water, so as to occupy the fourth part of its capacity; he then suspends a certain quantity of feathers perpendicularly, the barrel lowermost, and so placed, as that its extremity only may touch the surface of the water; he then covers the kettle with a lid properly adjusted, boils the water, and keeps the feathers four hours in this vapour bath. By means of this process he frees them of their fatty parts, and renders them soft and transparent. On the following day, after having scraped them with the blade, and then rubbed them with a bit of cloth, he exposes them to a moderate heat. By the day after, they are perfectly hard and transparent, without, however, having the inconvenience of splitting too easily,—*Neues Kunst und Gewerb-Bl. April* 1825.

Method of using pure Muriate and Sulphate of Soda, in the Manufacture of Glass, by M. Leguay.—Muriate of soda and sulphate of soda, may be employed, and at times with advantage, in glass-making. A casting is readily obtained of very fine glass, having, when about three or four lines in thickness, a very slight green tinge. Its composition is as follows: decripitated muriate of soda, 100 parts; slaked lime, 100; sand, 140; clippings of glass, of the same quality, from 50 parts to 200. Sulphate of soda likewise offers a great economy in its employment. The results are very satisfactory. The glasses made with this salt were of a very fine quality. The following is the composition: dry sulphate of soda, 100 parts; slaked lime, 12; powdered charcoal, 19; sand, 225; broken glass, from 50 to 200. These proportions give a rich coloured glass, which may be employed with advantage in glass-houses, where a fine quality is sought after. The following is the second way of operating with sulphate of soda; the proportions may be as follows: dry sulphate of soda, 100 parts; slaked lime, 266; sand, 500; broken glass, from 50 to 200. According to this process, it is obviously easy to operate in a regular manner, and to avoid expensive trials in the manufacture.—*Annales de l' Industrie Nationale.*

On Rinmann's Green.—This colour is an intimate mixture of the protoxide of cobalt and the oxide of zinc, which assumes a very lively green tint, after it has been heated to redness. In order to form this green, suddenly, as if by the eruption of a volcano, mix together two parts of nitrate of zinc, and one part of subacetate of cobalt, and expose the mixture to a spirit of wine lamp in a glass globe, with a short neck. This mixture soon becomes liquid, and appears at first of a rose-red colour, then purple, then blue. In an instant it inflames, detonates, becomes dry, and assumes a green colour. The product is scattered upon the vessel in the form of small rolled leaves of tea.—*Bull. des Sc. Nat.*, May 1824, p. 292.

Singular Phenomena observed by M. Ramond on the Pic du Midi.—When M. Ramond was on the Pic du Midi, he observed his own shadow, and those of his two companions, projected on a cloud situated a little distance above them, with a distinctness and an accuracy of outline quite surprising; but what was more astonishing, *these shadows were encircled with glories, shinning with the most brilliant colours.* "Those who witnessed this magnificent spectacle," says M. Ramond, "might have supposed that they were assisting at their apotheosis." Several naturalists, among others, Bouguer, and the sons of Saussure, have seen this phenomenon; but none of them observed this distinctness of form, which can only be explained by the smoothness of the surface of the cloud upon which the shadow was projected. With respect to the glory, Bouguer supposed that it might arise from the decomposition of the light produced by the particles of ice suspended in the cloud. Thus he would say, that the rays of the sun being intercepted at the place occupied by the shadow, there is produced at the place a coldness, and the icy particles becoming more numerous there, and on the margin of the shadow, produce the decomposition of the light. M. Ramond, however, objects to this explanation, and considers it as certain, that the cloud on which his shadow was projected could not, from the temperature of the Pic, have then held any icy particles in suspension.

On the Powerful Effect of Burning-Glasses at great Heights.—The extreme transparency of the air on high mountains, which hinders the calorific rays which traverse it from heating it directly, gives rise to several effects different from those we observe on the surface of the earth. The heat of the ground, for example, which absorbs the solar rays on those summits, is often, as M. Ramond observes, out of all proportion to that of the atmosphere. When these rays, therefore, are collected in the focus of a lens, they have much greater power than when they traverse a thick and less transparent atmosphere. He found that a lens of a very small diameter was sufficient to set fire to bodies, which a lens of double the diameter would scarcely heat in lower regions. M. Ramond supposes that the temperature of the different colours of the spectrum might be well ascertained on lofty summits.

The Memoir of M. Ramond, which contains these two notices, is entitled, *On the Meteorology of the Pic du Midi*, and was read at the Academy of Sciences, on the 13th of March 1826.—*Le Globe*, March 16, 1826.

Selenium in the Sulphur of the Lipari Islands.—M. Stromeyer has discovered Selenium in sulphur from the Lipari Islands, alternating in white and brownish orange layers, with sal ammoniac. It is probable that the orange tint of the sulphur arises from the Selenium.

Prussian Blue in the Soda of Sicily.—In three parcels of soda from Sicily, M. Brandes has obtained Prussian blue. When he heated the soda with warm water in a white iron vessel, he obtained much more of the Prussian blue than when he dissolved it in a porcelain dish. *Arch. des Apoth. ver.* 1822, No. 3. p. 215.

Celestial Phenomena for the months of September and October, 1826, computed for Cambridge, in lat. 42° 28′ 28″ *N. and long.* 71° 7′ 25″ *W.* Mean time. Communicated by R. T. Paine, Esq.

September, 1826.

D.	H.	M.	S.		
9					*Occultation* of 1 μ ♐.
"	8	55	50	P. M.	Conjunction in the Ecliptic.
"	9	42	27	"	Immersion 15′,7 } North of
"	10	10	04	"	Emersion 13,1 } ☽'s centre.
10					*Occultation* of *d* ♐.
9	11	20	57	"	Conjunction in the Ecliptic.
10	0	10	16	A. M.	Immersion 1′, 5 N. of ☽'s centre.
"	0	41		"	Moon sets.
14					*Occultation* of Situla.
"	7	28	02	P. M.	Conjunction in the Ecliptic.
"	6	18	44	"	Immersion 0′, 3 } S. of ☽'s
"	7	26	56	"	Emersion 1, 1 } centre.
16					Great. Elong. of ☿ W. of ☉.
18	4			A. M.	☌ ☉ and ♃.
23	9	47	30	"	☉ enters ♎, Autumn begins.
28					♅ stationary.

29	7			A. M.	□ of ♄ ♑. of ☉.
					October 1826.
2	9	37	59	"	✱ ☽ and ι ♍.
12	7			P. M.	Sup. ☌ of ☿.
13					Greatest Elong. of ♀ E. of ☉.
13	10			P. M.	□ of ♅ E of ☉.
15					*Occultation* of π ♓.
"	9	47	56	P. M.	Conjunction in the Ecliptic.
"	8	47	4	"	Immersion 4′,0 } South of
"	10	0	36	"	Emersion 5, 8 } ☽'s centre.
18					♄ Stationary.
19	5	17	20	A. M.	Im. 2d. Sat of ♃.
20	6	9	51	"	Im 1st. Sat. of ♃.
21					*Occultation* of 1 χ Orionis.
"	2	56	36	A. M.	Conjunction in the Ecliptic.
"	1	47	19	"	Immersion 0′,5 } South of
"	3	16	50	"	Emersion 0,9 } ☽'s centre.
23	5	48	9	P. M.	☉ enters ♏.
26	4	49	59	A. M.	Em. ♃ 3d. Sat.
30	8	21	6	P. M.	☌ ☉ and ☽ ☉ partially eclipsed, invisible.
31					*Occultation* of Mercury.
"	3	57	44	P. M.	Conjunction in the Ecliptic.
"	4	15	55	"	Immersion 9′,8 } North of
"	5	18	52	"	Emersion 3,4 } ☽'s centre.

The eclipse of the Sun of the thirtieth of October, will be visible to a small part of the earth, south of the antarctic circle, but so great will be the moon's distance from the node, that in no place, will more than one tenth of the sun's disc be obscured.

The eclipses of the satellites of Jupiter will be invisible during the greater part of the above months, on the account of the planet's proximity to the Sun; towards the end of October, however, three will be visible.

The occultations are uncommonly numerous; those which will happen on the evening of the ninth of September, will be very beautiful, as the stars are of considerable magnitude and the immersions will take place behind the *dark* limb of the moon.

Boston, July 25, 1826.

ART. XLVIII.—*Biographical Memoir of the late* HENRY KÜHL, Doctor of Natural History, &c. &c.*

HENRY KÜHL, was born at Hanau, on the 17th September 1797. His father, John Henry Kühl, was president of the provincial court of judicature, an office which he still continues to discharge. His mother, Maria Judith Walther, who died at an advanced age, was the daughter of Frederick William Walther, who was Counsellor of State at Hanover.

Kühl, who was by nature endowed with an excellent genius, was, from his earliest years, so fond of the study of natural history, that he usually devoted to it all his spare hours after the ordinary labours of the school had been performed. He happened to be born at a time and in a country which were particularly favourable to the cultivation of his genius; for no one will deny that the discoveries, by which many parts of natural history have of late years been elucidated, are in no small degree to be attributed to the naturalists on the Mayne.

In the number of these were many friends of his father, such as Leisler, Meyer, Gærtner, and De Leonhard. The two former introduced him to the study of geology, Gærtner to that of botany, while Leonhard taught him that of mineralogy in general. But he was especially indebted to the care of Leisler, who, being without children himself, and knowing Kühl to be very fond of the study of natural history, and possessed of great talents, took him into his society, which proved of the greatest advantage to him. For Leisler was president of the association which had been formed at Hanau not long before, for the sale and exchange of objects of natural history, and an opportunity was thus afforded him of handling and examining these objects. He himself collected new specimens, for the museum, in the surrounding country, and transmitted them in a sufficiently finished condition; and besides, freed his friend of much of the labour to which his literary connexions subjected him. He met him daily at those hunting excursions which proved of so much benefit to the ornithology of Europe, accompanied him on these expeditions, and, in short, was conducted by him into the only path by which the assiduous investigator of nature can be led to acquire a true knowledge of the objects of his pursuit. The consequence was, that Kühl, while yet a boy, was much better acquainted with these objects than often falls to the lot of people considerably advanced in years. In this manner he obtained a much more complete knowledge of the natural productions of the country of Hanau than could have been expected at his early age; and, at the same time, so improved the natural acumen of his mind, that he afterwards detected, in other parts of the earth, objects which had eluded the observation of many naturalists.

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* Prepared from the Memoirs of the Academia Cæsarea of Leopoldino-Carolinæ.

Without allowing their proper weight to these circumstances, it will scarcely be credited that Kühl, when only nineteen years of age, had already gone over the whole range of mineralogy under the instruction of Leonhard, examined a great number of the plants growing in Wetteravia, with the assistance of Gærtner, and acquired a knowledge of all the quadrupeds, birds and fishes, of the middle parts of Europe. Besides, at this age, he published, in the Wetterauen Annalen, vol. iv., a paper on the bats of Germany, in which several species were described for the first time by himself. After Leisler's death, which happened on the 18th November, 1813, he undertook the charge of the zoological department of the institution mentioned above, for the sale and exchange of natural curiosities. Nor were his scientific pursuits interrupted by the accession of these new labours; for although he managed nearly the whole business himself, he so assiduously cultivated them as to be taken notice of by the most illustrious naturalists of our time.

After having undergone the usual preparatory exercises in the Latin school of Hanau, he determined to proceed to the University of Heidelberg, with the design of devoting his whole life to the study of natural history, and the resolution of patiently submitting to all the inconveniences which, from the want of sufficient pecuniary resources, he forsaw could not be avoided, in the pursuit of this science. On this occasion he writes in the following manner to his friend Bojes: "What literary profession I shall follow, I do not know. This I know, however, that, without the study of natural history, I cannot live. I therefore wish, with all my heart, that whatever situation I may have in future, it may leave for me a few by-hours, in which I may indulge in those investigations which are of all others the most agreeable to me. I do not seem to have been born for the study of law, which some recommend to me to follow; and so I imagine my best plan will be to study medicine. But I should gladly renounce this also, the moment an opportunity might occur, that I might give myself wholly up to natural history. But if this do not happen, I shall accommodate myself to circumstances, and study medicine, for physicians are required all the world over; and when my studies are finished, I shall endeavour, if possible, to get out to America, or wherever fortune may lead."

He, therefore, in the month of September, 1816, had made up his mind to go to Heidelberg, when Theodore Van Swinderen, one of the Groningen professors, becoming acquainted with him, prevailed upon him to follow him to Groningen. This arrangement was fortunate for Kühl; for although his genius and assiduity would have led him to eminence, independently of Swinderen, yet this object could only have been attained by a longer way, and after much time. For with what difficulties would he have had to struggle, and how many sources which Holland disclosed would have been lost to him? But here, under an entirely different sky,—in the midst of other plants and other animals,—in the vicinity of the sea, which he had long wished to behold, and access furnished to the rich collections of Holland,—he daily saw new objects to excite afresh his thirst after knowledge. In short, he there found opportunities of improvement which few other places could better afford.

Difficulties, however, occurred, which presented obstacles to his departure for Holland; but these were removed by the kindness of Swinderen, who felt much interested in him, and, moreover, was influenced by the hope that he would prove an honour to the university of which he himself was a member. At the end of September, 1816, Kühl arrived at Groningen, and from this time they pursued their studies together. It happened that Swinderen had then commenced his lectures on Natural History, which being on a larger scale, usually occupied four years in continuance, whereas his ordinary lectures were finished within a year. Kühl, therefore, was admitted into the number of his hearers; nor was he merely a hearer, but also an assistant. This year, although he attended De la Faille and Bakker on natural philosophy and anatomy, he was chiefly occupied in the study of natural history, confining himself to the mammalia, and making choice of a more precise method in treating these different subjects, than is usually followed. With regard to Swinderen's lectures on quadrupeds, this is sufficiently attested by his prize essay, in which he very ingeniously explained the gradual manner in which the animals of this class pass into each other. There needs be little wonder, then, if he gained the gold medal.

Previous to 1816, the year in which he came to Groningen, the zoological knowledge which he had acquired, included only indigenous animals, as he had seen but very few exotic ones. On this account, however accurately he knew indigenous animals, and their mode of living, he yet wanted some general prospectus, without which there can be no order in the study. He, therefore, first of all read Illiger's Prodromus, which Swinderen followed in his lectures on quadrupeds and birds. But he did not confine himself to merely reading it, but at the same time diligently compared each description with the object itself in the museum. In this manner, during the first period of his residence here, he passed five days of the week; but on Saturday he made an excursion to this or that village along the coast, in order to collect fishes or birds. On the winter holidays of this year he made journeys to Amsterdam, Harlem, and Leyden, in which he visited Temminck, as well as the elder Voigtius, who sold him many natural objects, among which were several that he had never seen before. He also met with other learned men, such as Brugmans, the Leyden professor, Van Marum, a medical practitioner at Harlem, and others. He also stayed several days in the town of Lisste, with Temminck, in order to collect the animals of that district, and dissect them.

During the Whitsuntide vacation, he made an excursion to the island of Rottus, where he not only obtained many natural objects, but also made an agreement with the keeper of the island, that he should transmit to him whatever might chance to be cast ashore upon it. The consequence of these journeys was an abundant supply to almost all the collections in Germany of seals and rare marine birds.

When he had returned home during the summer vacation of 1817, he acquired a knowledge of many expensive works in the library of the Society of Wetterau, and, during the same vacation, he made a trip from Hanau to Heidelberg, where he met with Tiedemann, a very accomplished and learned man, who favoured him with useful advice regarding his studies, and presented him with some of his celebrated works. By this time he had acquired so eminent a name in his own country, that he was elected a member of the Natural History Society at Marburg, as well as of that which has been instituted at Hanau, under the name of the Wetterau Society, for promoting the study of every department of Natural History. And, in the end of the following year, on the 20th December 1818, he was made a member of the Academia Cæsarea Leopoldino-Carolinæ, under the name of Johnson.

On the 12th August he passed the Rhine, accompanied with a large party, in which were Leonhard and his family, and returned to Groningen, by Newied, Duisburg, Utrecht, Leyden and Amsterdam. At Newied he was introduced to Prince Maximilian, lately returned from his journey through Brazil, who received him very graciously. In the same manner he was received at Utrecht by Professor Fremier. Passing through Leyden he so gained upon the regard of Brugmans, that after he had taken him through the public musuem, and shewn him the collection of natural objects which he possessed, as well as his splendid library, he frankly offered him twice the salary which he had at Groningen, if he would remain at Leyden. What an inducement to a young man, inflamed with the desire of knowledge! The double salary was less an object of importance with him; but how great the difference between the Leyden musuem and that at Groningen, between the Leyden apparatus and that which the Groningen University at that time possessed! between Brugmans' library and the one at Groningen! How great the conflict between the love of knowledge, and the desire of discharging his duty! But duty prevailed: "I am too much bound to Swinderen," he replied, "to remain with any other than him, during my stay in Holland." So, in the beginning of September, he returned to Swinderen, like a new gift. He received a gold medal from the faculty of Groningen as the reward of his industry, and was admitted by the Physical and Chemical Society into the number of its members.

In the second year which he spent at Groningen, he attended the lectures of Driessen on Chemistry and Botany; of Bakker and Wilkens on vegetable and human anatomy and physiology; of Swinderen on the Natural History of Birds, and in this department, of which he was particularly fond, and which he had already assiduously cultivated, he was frequently not merely a hearer, but also an assistant, as Swinderen himself testifies. Besides, he turned his attention to the history of exotic plants and animals, as well as to comparative anatomy; while at the same time he dissected and diligently described the whole of the indigenous birds and fishes, and defined the coloured figures of birds published by Buffon.* The discoveries which he made in dissecting the various kinds of animals, were afterwards published in the second volume of "Additamenta."

During the long vacation of 1818, he made a journey on foot through Germany with his friend Van Hasselt. They left Groningen on the last of June, and already on the third day after their departure had reached Bremen, thirty-six hours' journey distant from the city of Groningen, where they were kindly received by Albers, Treviranus and Mertens. In the musuem of this city, there was a remarkably good zoological collection, and Albers' private cabinet was well stored, particularly in comparative anatomy. Kühl took notes of every thing that he saw. From Bremen they went to Brunswick by Cella, and there they visited the Duke's collection, which is very rich in preparations of amphibia, both anatomical and osteological, while it is at the same time well supplied with birds. From thence they set out without delay for Berlin, where they arrived on the 2d or 3d of July.

Immediately after their arrival, they visited Lichtenstein, whom, having known two years before, they found a very accomplished man, as well as a very learned zoologist. "He treated us," says Kühl, in a letter to Swinderen, "as if we had been his own children; and whenever we visited the Royal Museum, or Lichtenstein's own library, we commonly supped at his house."

In this manner they spent some weeks in Berlin, applying to their studies with the utmost diligence; and the last days of their stay were further improved to them by the arrival of the celebrated Temminck. From thence they proceeded to Halle, where they met Professor Nitzsch, and other naturalists; to Leipsig, Dresden, and into German Switzerland, and, lastly, to Jena, being every where received in a friendly manner.

Kühl also paid a visit to Naumann upon this journey, and the respect which he had before entertained for this celebrated ornithologist, was by no means diminished by his personal acquaintance with him. As the time allotted for their excursion was now drawing to an end, they spent the remainder of the vacation in the house of Kühl's father, making a short journey to Heidelberg, where Tiedemann gave them very useful advice, with regard to the manner in which comparative anatomy should be treated.

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* Buffonii et Daubetonii Figuarum Avium coloratarum nomina systematica. Groningæ, 1820.

From thence they returned to Groningen, on the 21st of September. In this year Swinderen read his fuller lectures on the cold-blooded animals, and the three great divisions of the invertebrata, in which he very ingenuously confesses that he was not less studiously assisted by Kühl in respect to mental than in manual labour. As an example of his industry and proficiency in this department, we may mention that he described the Kobelian Collection of Insects, which was purchased at this time, in such a manner as few collections in Europe have been described.

It is also due to him to state, that he gave excellent definitions of most of the amphibia depicted in Seba's Thesaurus, by which means Swinderen's copy is rendered of the greatest value; and Swinderen further states, that, at the request of several of the most eminent naturalists, he described many things relating to this explanation of Seba's plates, and transmitted them to them; and it would be of advantage to science, as well as creditable to our author, that this work were published, since the Index to Buffon and Daubenton is already before the public. But it is a matter of much regret, that Kühl never completed the plan which he had entered upon, of defining the whole of Seba's work; from doing which, as well as from composing many other excellent works, he was prevented by his journey to India, and ultimately by his premature and lamented death.

We have thus come to another and very important period of his life, to the time when he prepared for his journey to India; which happened in the following manner.

Having gone, in his accustomed manner, during the short vacation of the Christmas season, to Holland, in order to inspect the specimens of objects, with which he was only acquainted by means of descriptions, on account of the limited nature of the collections at home, he betook himself to Temminck, at Amsterdam, with the view of defining, along with him, the collection of fishes which he had either brought home himself in the preceding summer, or had received, from the Mediterranean. And being requested by this celebrated naturalist to undertake along with him the description of his splendid ornithological collection, he undertook a selection of the Index published by Latham, as a prodromus of his General Synopsis of Ornithology, a work of much labour; by which Kühl, with the guidance of Temminck, and the use of his admirable library, indeed made remakable progress in the knowledge of exotic species, but which, being deficient in the lately discovered species, has yielded to the *Planches colorées*, which are now published by Temminck and Laugier.

While Kühl was thus occupied at Amsterdam, Swinderen received a letter at Groningen, by which intimation was given of his being appointed to undertake a journey at the public expense. A letter was also transmitted to Kühl himself, by the King's minister and counsellor for public institutions of teaching works of art, and the colonies, to the following effect: That his Majesty from what had been reported to him of his knowledge in the various departments of natural history, his love of science, and the ardent desire which he possessed for the extension of knowledge, had selected him, as a young man, who might be sent with advantage to one or more of the colonies belonging to the state, in order to lay before the learned of his own country, and of Europe in general, the still hidden treasures which nature had so largely bestowed upon them.

Nearly at the same time, the minister, thinking it absolutely necessary, in order to insure the success of the undertaking, that Kühl, before setting out, should be well acquainted with the full progress which science has made in Europe, persuaded the king to order him to proceed to London and Paris, at the public expense. This intelligence produced the most lively emotions in the mind of Kühl, and, in a letter to Swinderen on this occasion, he writes as follows:—"I was so agitated by the message, that I did not know what to do for joy. Much, indeed, am I indebted to you and Temminck, nor shall I ever forget the many good offices which you have performed to me. It was always, indeed, my firm resolution to devote myself entirely to science, but with how many impediments should I have had to struggle, had I not been so fortunate as to become acquainted with you. My residence at Groningen was among the most agreeable periods of my life, and will always remain so; nor shall I ever forget the happy days in which I there commenced my studies."

On the 3d of April, he set out, accompanied by Tremminck and Lichtenstein, for London, where he was kindly received by all the learned. Almost all of them presented him with specimens, or at least gave him free access to their collections, that he might have an opportunity of drawing or describing whatever he deemed of importance. He there entered minutely into the study of the mammalia, birds and amphibia of New Holland, the description of the natural history of which, having already collected much with regard to it at Berlin and Amsterdam, he had in view to finish the following winter at Paris. He found a great abundance of these animals in London, as the Linnean Society was in possession of a very extensive collection from New Holland, which had not previously been described. In the course of a few days he described and arranged 200 of these animals. He also found much useful matter for his monographs on the genera Falco and Psittacus (the former of which, however, has not been published); and acquired a knowledge of all the objects which had been brought home by the last polar expedition.—The Banksian Library, which contains an immense collection of books on natural history, being open to all who profess that science, was visited by him between the hours of ten and four; and, among the sketches made by Forster and Sir Joseph Banks himself, he found many things of great importance. In this library he also began the compilation of an Indian Fauna, which he intended to make use of in the colonies which he was about to visit.—Access being given him to Bullock's Museum, which was then proposed for sale, from ten to five, he attended during these hours, to acquire a knowledge of the specimens in that great collection, and describe and name what was new. With regard to mammalia, this museum possessed few that were not previously known; but of birds he described upwards of 100 new species, and corrected the descriptions of a great many more.—He experienced much kindness from Mr Ayton, the manager of the botanical garden of Kew; and also met with Decandolle, who was then consulting the herbaria of Smith and Brown for his great work. In the Banksian Museum he became acquainted with a gentleman, who having commanded the British army in America, had there collected a great store of zoological subjects; and this acquaintance turned out of much importance to him with regard to the object of his journey, for this gentleman was enabled, from his own experience, to furnish our traveller with circumstantial directions in respect to the mode of travelling in hot countries, and was of much assistance to him in procuring the articles necessary for his journey.

Before leaving England, Kühl made an excursion, along with Laugier and a Breslau student, through the south of

England, visiting Oxford, Windsor, Bath, Bristol, Severn, Southampton, the Isle of Wight, and Plymouth. Wherever he came to the sea, he collected natural objects; and many beautiful shells, both terrestrial and aquatic, were given by him to the Groningen Museum, as a memorial of his journey.

During the short time which he spent in Holland, an honour was conferred upon him which is seldom given to students, he having been promoted to the degree of Master of Philosophy and Doctor of Natural History, on the 6th August; as an acknowledgement for which, he presented to the Museum of the University four new genera of birds of the rarest kind, Pozophorus, Melliphaga, Menura and Aptenodytes.

In the beginning of September, accompanied with his friend Cremersius, whose loss we have now also to deplore, and with Van Hasselt, he returned by Brussels, Naumur, (where he met with Professor Galdius,) Aix-la-Chapelle and Newied, (where he consulted Prince Maximilian with regard to his intended journey) to Hanau, where he remained with his father for some time, while his friends Cremersius and Van Hasselt made an excursion through Switzerland.

In the beginning of November he left his father and went to Strasburg, in order to meet his friends there, according to promise, and continue his journey with them to Paris. The end which he had in view, in undertaking this journey, was to study the invertebrate animals a second time, under the direction of Cuvier and Lamarck; to examine the herbarium made a short time before in Java by Leschenault, and hitherto known to few; but, above all, by making use of the rich collection of amphibia in Paris, to bring more quickly to a conclusion the system of amphibia which he had been busied in for two years back.

Besides, as he had seen all the collections in Berlin and London, he was anxious to inspect those which yet remained in abundance in Paris. For which purpose, Kühl and Van Hasselt having taken lodgings near the museum of Natural History, spent almost the whole day there, in examining all the genera of invertebrate animals which they found. At the same time, they read in the evenings at home the works of Cuvier and Savigny, on the anatomy of these animals, and received various objects for examination from the former; and, besides, Kühl occupied those hours in which Van Hasselt and Cremersius attended the medical lectures and hospital, chiefly in collecting materials for his work on amphibia, birds, and other animals.

The distinguished Cuvier also, had a literary party on the Saturday evenings, to which all the learned men residing in Paris were usually invited, and Kühl and Van Hasselt were constantly invited to attend. These were also allowed free access to Cuvier's library, and were permitted to work in the same apartment with him; while orders were at the same time given to the different keepers of the museum, to open all the cases to them when required.

But, without doubt, among so many illustrious men, he was most disposed to pay the tribute of admiration and esteem to the celebrated Humboldt, a man equally noted for his benevolence, learning and prudence. Humboldt procured for Kühl and his friend free access to the Royal Institute, by which they were allowed to attend the sittings and make use of the library; besides, he very kindly offered Kühl the use of his own library, and did not think him unworthy of being admitted among the number of his particular friends. During his residence at Paris, Kühl completed his monograph on the genus Psittacus, which was printed in the tenth volume of the Transactions of the Leopoldine Society.

Leaving Paris on the 26th February, 1820, he returned straight to Hanau, where he laboured with indefatigable diligence in correcting his manuscripts, so as to be able, soon after, to send to the press, his first volume of "Additamenta ad Zoologiam et Anatomiam Comparatam." After reducing all these to order, he took leave of his father and family, and proceeded to Amsterdam, in order to make preparations for his journey; this done, he went to Groningen for the last time, and after staying there one day, bade adieu to his friends. On the 10th of July, 1820, the ship in which he embarked, commanded by Breukcmeyer, sailed from the Texel.

When in the Channel, they examined various species of flexile polypi, and dissected different fishes which do not occur on the coasts of Holland. They noticed a new genus of the family of corallines, and found abundant matter for investigation in the numerous species of flustræ and fuci which occurred. Among other remarkable fishes they dissected the following: Scomber scomber, Conger conger, and Raja oxyryncha. In the Bay of Biscay, the weather was so bad that they could not collect anything. In the Spanish sea they caught the first salpæ, and among them several new species, as well as a genus allied to these, to which they gave the name of Selenosoma.

On the 18th July they anchored at Madeira; and, although five days only were allowed for their stay on this island, they examined much of its natural history. They were kindly received by the English governor, and, with his advice and assistance, were enabled not only to examine the shores, but also to penetrate into the interior of the country. This gentleman, whom Kühl happened to meet on his journey, being informed of the object of their voyage, requested him and his companions to come that same day to his seat, two miles and a-half distant from the town, and high above the level of the sea. Having rested here for some hours, they took their departure, and ascended the mountain named *Pico Rufo*, and found its height according to Hasselt's measurement to be 2500 feet. They collected a great many plants in these five days, the number of which they have stated at 224, although this district is not by any means considered as fertile in vegetable productions. They also inquired into the distribution of the plants at different altitudes, and looked after the geological structure of the island. In the animal kingdom, the principal productions that occurred were insects; for this country is very deficient in animals of the higher orders. They found no mammalia, but Kühl discovered a new species of bird, nearly allied to Fringilla cœlebs. Besides, they found a great number of amphibia, but all belonging to two species of lizards, as well as many fishes, but all of six genera; nor did they meet with any fresh-water fishes.

Leaving Madeira on the 3d August, 1820, they entered the tropical seas, where so many objects often occurred in a single day, that they were overwhelmed by their multiplicity. There occurred in particular a vast multitude of remarkable and partly new mollusca echinodermata, and entozoa; and there they made many important observations, with regard to the conformation of various fishes, mollusca, aud radiaria.

On the 9th of October they made the Cape of Good Hope, and tarrying there twelve days, explored the mountains or shores from the first dawn to late in the evening. They carried with them the skins of various birds prepared for stuffing, and also preserved a great number entire for the purpose of making skeletons. They also prepared skeletons of the *Viverra genetta* and *Chrysochloris capensis*. Besides, they collected amphibia, fishes, mollusca, crustacea, radiaria, and many species of fuci, in Table Bay and Hout Bay. The number of plants which they collected at the Cape, they had not determined at their departure, but they mentioned it as being very great, they having come there in the proper season. They also prepared some bundles of bulbous roots, and the seeds of about 200 species, for the Botanical Garden at Batavia. But what is particularly worthy of remark is, that they examined with great care the structure and stratification of Table Mountain, and the other hills which surround Table Bay and Hout Bay, concerning which very erroneous ideas had been entertained.

The Great Indian Ocean, not less than the Atlantic, furnished them with ample opportunities of scrutinizing animals hitherto but imperfectly known. They discovered several genera of the family of naked mollusca, annularia, and tunicata of Lamarck. Bad weather at length forced them upon the Cocoa Islands, toward the south of Sumatra,—a group, according to their account, entirely composed of madrepores, and perfectly similar to those flat islands of the Pacific Ocean, whose origin has also by Forster been attributed to madrepores.

On account of the multitude of sharp coral rocks, access can only be had to the bay of these islands in fine weather, and with very small vessels. Their origin from corals shooting up from the bottom of the sea, is plainly demonstrated by their want of mammalia, amphibia, and land-birds, and the scantiness of their flora, which was found to consist of only four species, a new grass, an urtica, the cocoa-nut-tree (*Cocos nucifera*,) a wood of which covers the interior of the island, and the *Tournefortia argentea*, which margins the cocoa-wood, with its dark green foliage. Innumerable bands of aquatic birds inhabit these desert shores, and so fearless were they, that many of them were caught by the hands, or killed with sticks. Amid all this profusion, however, there were but very few different species.

They first landed in Java at the Promontory of Banta, and after being a short time on shore, returned to their ships, loaded with corals and mollusca. The labours and inconveniences of their voyage were amply compensated by the kindness with which they were received, and the liberality with which they were treated, by his Excellency the Governor of the island. He permitted them to live at Buitenzorg, in order to become more readily accustomed to an Indian climate, from the salubrity of the air in that place. The first four months they passed near Buitenzorg without interruption; for since they daily found an abundant supply of new objects for investigation, it would have been useless to extend their excursions, and seek at a distance what they could obtain at hand. Not a day passed without their naming, describing, and drawing some new species or genera, or even orders. In this manner they prepared a Conspectus of the Flora and Fauna of Java, taking care not to waste their time in delineating what had already been described by Reinwardts. On which account they made out an index of all the drawings which he had made, inspected his Herbarium, and received many of the names which he had imposed. In the course of these four months which they occupied in collecting natural objects, they had done so much, that, by the 10th of August, they were able to write home to the minister, "that their labours regarding some of the objects of investigation in this country were now finished, and that among these might be enumerated the Cheiroptera, Ophidia, Sauria, Birds, Fishes, and Mollusca." At this time they were of opinion, that, with regard to the Buitenzorg Fauna, they were as well acquainted with the animals of this district, as with those of any country in Europe. Having made this foundation, they proposed making a journey to Banta, a less known part of the island, when the cholera made its appearance, and frustrated this design. On which account they set about occupying the following months in ascending the neighbouring mountains, especially the summit of the mountain Salak, 4550 feet in height, situated above Buitenzorg towards the east, one of whose summits had already been ascended by Reinwardt, although a higher one toward the west had been left by him, and had consequently been hitherto unattained. They then came upon a continuous ridge of mountains, extending northwards toward the sea-shore, and among others ascended the ridge of Munar. After this they visited the three hot saline springs, situated between the villages of Rompin and Waru (which are not marked in our common maps,) arising from these calcareous hills, which seem to have been themselves produced by the deposition of calcareous matter from the water,—a process which is still daily taking place. An entirely different vegetation was found in the vicinity of these springs, and as it was probable no person had hitherto examined it, they were highly delighted with it. On the 10th August they returned from the mountain Pangerang, 8580 feet in height, forming on one side a continuous ridge with the mountain Gedé, which they had ascended with much labour. These summits, together with that of Salak, situated towards the east, were volcanic. In these pretty elevated and cold regions, they found many retreats of rhinoceroses, and the paths which these animals had made, afforded them considerable facility in ascending the mountains. So great was the profusion of natural productions in these higher regions, that, notwithstanding their unremitted attention and industry, they could not manage to describe the whole. Amidst almost insuperable difficulties, and exhausted with labour, they at length reached the summit. But after they had got to their huts in the evening, a violent rain came on, which continued for three days, and as nothing could resist its impetuosity, they were miserably affected by the cold, and this the more especially, as they had been by this time familiarized to a hot climate.

For the first fourteen days, however, after this excursion, Kühl enjoyed very good health, on which account they supposed that he had escaped the danger, and rather rejoiced that they had been subjected to it, as they imagined the worst was past, and expected to be safe in future from all attacks of the climate. But soon after Kühl became affected by a disease which resisted all treatment, accompanied by inflamation of the liver to such a degree that his life was in danger. He saw from the first that the disease would prove mortal, but he waited the approach of death with becoming fortitude. "The tranquillity of mind which he possessed when in good health," writes Van Hasselt, his companion, to Swinderen, "instead of being diminished by sickness, was rather increased. I have been astonished at the calmness with which he spoke of his approaching death. He even gave me some injunctions about things which he wished to be performed before he died. In fact, I am not less an admirer of him, now that he is dead, on account of the fortitude which he displayed, than I was his friend when alive." After having laboured four weeks under his disease, he died on the 14th of September, 1821, not yet twenty-four years of age.

Kühl was of an excellent disposition, and the most refined manners. He was quick of comprehension, acute in discriminating, and possessed of a most tenacious memory,—qualities of the utmost importance to the naturalist, especially as 'the compass of science is now so extended, that, without a proper arrangement of ideas, and a faithful memory, the various objects cannot fail to be confounded. Nor was his manual dexterity less remarkable than the acuteness of his intellect: he could in fact do every thing with his hands that he wished; he stuffed the skins of animals, dissected them with great neatness, and drew not less beautifully than accurately, so as occasionally to supply the place of a painter, on the voyage undertaken to the island of Java, in delineating anatomical subjects.

To these qualities Kühl added the greatest industry. With him every moment of time was employed: when in his room he was either reading or examining natural objects; when walking, or upon a journey, he was collecting animals, plants, and minerals, and thinking upon their qualities and properties. He deemed it base to enjoy repose longer than was necessary. He often sat up at his studies till midnight; and when Swinderen shewed him his bed-room on his arrival, the first thing he did was to see whether a bell might be hung above his bed, that the watch might waken him every morning at four, by pulling at the door, in such a manner, however, as not to disturb the rest of the family. A man possessed of so much genius and assiduity could not therefore fail to acquire a vast stock of knowledge. Nor was his knowledge confined to natural history alone, but also extended to political history, both ancient and modern. He was also so well versed in geography and all other attainments requisite in a well educated man, that, even at the time when he came to Groningen, the most accomplished students, on hearing him converse on general subjects of literature, acknowledged with one voice, that, in the course of their studies, they had met with no young person equal to him. And to this genius and this industry were added an ardent mind, which prompted him to great undertakings, a constancy which sustained his efforts, and a devotion to science which made him prefer it to all other objects.

It has already been observed, that natural history was his favourite pursuit; and there are none or at least very few examples, in any other department of science, in which so much keen investigation is displayed, so many dangers so fearlessly confronted, and so many privations borne with fortitude, as we see evinced in the character and conduct of the disciples of nature.

All these essential good qualities were combined in Kühl. Besides the other virtues with which he was adorned, he possessed a remarkable moderation in regard to food. On his journeys he required nothing more to allay hunger and thirst than dry bread with water and milk, provided he could attain the object to which all his labours were directed,—the extension of his knowledge. To accomplish this he left his country, and friends; in its pursuit exposing himself to the dangers to which a long sea voyage, change of climate, untrodden paths, savage men, and wild animals exposed him; and all these he underwent without the prospect of any great reward, at least without the hope that these labours would be repaid by greater advantages than his accomplishments and celebrity had already acquired for him in Europe. Certain it is, that it was neither by the hope of riches, the most general stimulus to exertion, nor the honourable and truly royal liberality with which he was equipped for his journey, but by the mere love of natural history, that he was actuated,—a truly sublime affection, which made him spare no labour in collecting new objects of investigation.

When he was last at Hanau, anticipating the difficulties which he had to encounter, he thus wrote to Swinderen: "I have determined to make a journey to the eastern countries, and go I shall, if there be no other way, even in the capacity of a barber." Such was the love of Kühl for his profession.

But Kühl had also other properties, which, however becoming in his person, were especially adapted for the department of science on which he had fixed his affections. Besides his other virtues, he was distinguished by an ingenuous mind. Truth, obedience, modesty, and a grateful remembrance of benefits, were among the ornaments of his mind. He willingly accommodated himself to the manners of his friends; but when persuaded that he was better acquainted with a scientific subject under discussion, he défended his opinion with warmth, although with modesty, nor yielded rashly to the determination of others. His filial piety, and the affection which he bore to his brothers and sisters, were highly exemplary; and it is to be mentioned to his honour, that he would receive nothing from his father, although it would gladly have been given him, lest he should diminish the portions of his brothers and sisters.

Kühl's greatest merit was, to have embraced the whole compass of natural history. For when he came to Groningen in 1816, he already knew a great number of minerals, both oryctognostically and geologically, the whole of the plants indigenous to the fertile district of Wetterau, and all the mammalia, birds, and fishes of the middle parts of Europe. He chiefly studied exotic plants in the Botanical Garden at Groningen, as well as in the other gardens which he saw on his journeys; and investigated the foreign vertebrated animals in the Groningen Museum, and in those of Berlin, Paris, London, &c., while he principally studied the invertebrate animals at Paris. "I hope," says he, in a letter to Swinderen while in Java, "that when I return to Europe, I shall not be accused of cultivating one department only of science, as my aim is not merely to treat those parts well, of which I was fondest when at home, but I am also bent upon mastering all the other branches.

Another merit of Kühl's was, that, besides the study of natural history in the more limited sense, he paid attention to anatomy also, and to physiology, or rather biology. Although the study of physiology was of all others that which he preferred, he was yet free of a fault into which many naturalists fall, who, on account of the main object of science, which they take to be the determination of the general laws of nature, despise the aids of less elevated but subsidiary studies; whence it necessarily follows, that they always fall short of their object. But Kühl did better; he set about both kinds of study with equal diligence, and in this respect is the only naturalist who can be compared with Pallas.

Tiedemann, the celebrated anatomist of Heidelberg, when he met with Kühl on his journey to Paris, said, that "a more accomplished naturalist never before travelled;" and Temminck writes thus to one of his friends, "Science, by the death of Kühl, has lost another Linnæus."

But even these were not Kühl's only merits, but rather the beginning and foundation of greater excellence; for he not only knew what others had done, but contributed as long as he lived to the improvement of science. Without insisting much upon his Annotations, not yet published, his Fauna of New Holland and India, or his Monograph on the genus Falco, besides the discoveries which he made in Java, and which, as they have not yet been made public, cannot be judged of, we shall confine ourselves here to the advantages which he has conferred upon science by his publications.

With regard to Mammalia, he drew up a general conspectus of this class, in the essay mentioned above, for which he gained the gold medal. In his monograph on the Simiæ, a work which seems to be the most complete in regard to the number of species of any upon the subject, he has described 111 species, and among these several new ones, first defined by himself, as well as many others which had hitherto been merely named. In his monograph on the Bats of Germany, he made known three new species, and rectified many errors connected with the specific distinctions. In his Zoologiæ Auctaria, he has proposed a new genus, Saccophorus (the Mus bursarius of Shaw,) and made known various new species of Mammalia; so that before he left Europe he had already increased the list of mammalia by many new ones, to which he would without doubt have added many more in Java.

But of all the departments of natural history, Ornithology was that which he most enriched by his discoveries, most of which he made along with the celebrated Temminck. His index to the coloured plates of Buffon, which Swinderen published with his consent, is also of much advantage to ornithology. This work of Buffon, which comprehends the whole range of the birds known in his time, is much superior to most of the works on the same subject, and will always remain so; but of what importance would those figures be without systematic names? This deficiency, then, was supplied by Kühl. Besides, he wrote a monograph on the genus Psittacus, in which he included 200 species, a considerable number of them being first characterised by himself, or distinguished and named with the assistance of the Temminck. Moreover, he described in his Auctaria a new genus discovered by himself, to which he gave the name of Ptilonorynchus, and wrote a monograph on the difficult genus Procellaria. Lastly, during his journey, as well as in Java itself, he made many other interesting discoveries in this department, of which, however, we are as yet only acquainted with a small part.

With regard to the Amphibia, he has the merit of having added many names to Seba's figures in his Thesaurus, by which he has much facilitated a reference to that work. Many of his observations on Seba's figures are also contained in the additions already mentioned. We there also find critical remarks on Daudin's work on serpents, and very useful annotations regarding the number of the abdominal and caudal scuta of serpents, of which much use is now deservedly made for the purpose of specific distinction. Had it been his fate to have returned from India, he would assuredly have described the new species of amphibia which he had discovered, according to Merrem's method. With respect to invertebrate animals, he did not publish any of his discoveries previous to his Indian journey. Whatever, therefore, was found by him subsequently, was common to him and his companion Van Hasselt. In regard to comparative anatomy, Kühl made most of his observations along with Van Hasselt, and their papers on this subject are consequently common. These papers, indeed, chiefly treated of the whole first four classes of vertebrate animals; but we may mention one in particular as worthy of praise, which contains a disputation regarding the hearts of various animals. In them, as well as in the other writings of Kühl, many new facts are detailed. On their voyage to Java, although they paid most attention to the dissection of invertebrate animals, yet, by their industry, they also extended the anatomy of the vertebrate ones.

In regard to the vegetable kingdom, they paid particular attention to the laws of the distribution of plants over the surface of the earth, in respect to which they instituted many and very diligent investigations in the Island of Madeira, at the Cape of Good Hope, and in Java.

In mineralogy and geology we are indebted to them for an account of the geological structure of Madeira, for a better view of the South Cape of Africa, as well as remarks upon the Cocoa Islands, and, lastly, for various communications illustrative of the colony of Buitenzorg in Java.

The printed works of Kühl are the following:

1. Die Deutschen Fledermäuse (in the Annals of the Society of Wetteran). Frankfort, 1819.
2. Responsio ad quæstionem, ab ordine disciplinarum mathematicarum et physicarum propositam: Cum, licet naturæ corpora varia modo inter se differunt, ex hucusque cognitis observationibus tamen constare videatur, ita comparatum esse rerum naturam, ut lento quasi passu ab una specie ad alteram progrediatur, atque sic continuam quasi catenam efficiat, ex variis quidem annulis, in fine tamen junctis compositam, hæc catena, in Mammalium classe, demonstranda quæritur. (Annals of the of the Groningen Academy, 1816. 1817. Groningen, 1818.)
3. Conspectus Psittacorum, cum specierum definitionibus novarum, descriptionibus, synonymis, &c. (Nova Acta Physico-Medica Acad. Cæs. Leop. Car. Nat. Curios. vol. x, part 1, pp. 1—104. Bonn, 1820.)
4. Beitrage zur Zoologie. Frankfort, 1820. 4to.
5. Beitrage zur vergleichenden Anatomie von Dr Van Hasselt u. Dr Kühl. Frankfort, 1820. 4to.
6. Buffonii et Daubentonii Figurarum Avium coloratarum nomina systematica. Groningen, 1820. Fol.

ART. XLIX.—*Observations and Experiments on the Structure and Functions of the Sponge.* By ROBERT E. GRANT, M. D. F. R. S. E., F. L. S., M. W. S., &c.—[*Edin. Phil. Jour.*]

[Concluded from p. 439.]

ALTHOUGH a minute examination of the internal structure of the living sponge is obviously the most natural and necessary step towards discovering its mode of growth and generation, and consequently the place this substance occupies in the scale of beings, and is certainly that most likely to lead to the discovery of some more fixed and scientific principles for discriminating the species, than the vague characters hitherto employed; yet we can scarcely discover, in the writings of zoologists, since the time of Aristotle, any attempt to investigate its structure in a scientific manner. Although Pallas, Lamouroux, Lamarck, Schweigger, and almost every modern zoologist, have considered the examination of this animal, in its recent state, as still an important desideratum in comparative anatomy; yet the deficiency is generally supplied only by ingenious conjectures from the appearance of dried specimens, or by supposed analogies with other vegetable or animal productions, rather than by patient dissection of the animal in its natural state. Cuvier states in his Regne Animal (t. iv. p. 87,) that the sponge is a fleshy substance, possessing no axis, either calcareous or horny; although we shall find, that, in one great tribe of these zoophytes, with spicula of complex forms, the axis is entirely calcareous and soluble, with effervescence, in acids; and it is well known, that the horny axis, of several sponges, have been constantly employed in the arts since the time of Alexander the Great, if not since the period of the Trojan war. Professor Schweigger of Konigsberg, who examined these animals alive, principally at Nice, believes that their axis consists of fibres which possess a small degree of irritability, by which they gradually contract the dimensions of the animal, when it is irritated, and thus force out the water from its canals (*Beof. auf. N. R.* 1819, p. 33;) although, in his experiments, he could not excite them to the slightest perceptible motion; and in most of the known species these fibres are composed of minute siliceous tubes, which scratch glass and resist the action of the blowpipe. Lamarck, reasoning from mere analogy, maintains, that every species of sponge possesses distinct polypi, closely resembling those of alcyonia, projecting from its surface; and that these two genera of zoophytes differ only in the greater or less density of their gelatinous matter (*An. sans Vert.* t. ii. p. 348—9); although his countryman Jussieu, nearly a century ago, by desire of the French Academy, examined with a microscope the *Spongia ramosa*, fresh from the rocks on the coast of France, and reported, that he could discover no kind of polypi in that animal (*Mem. de l'Ac.* 1742); and the accuracy of Jussieu's observations has been confirmed on a great variety of sponges, by every succeeding observer, as by Cavolini, Lamouroux, Schweigger, &c. It was scarcely consistent in Cavolini to consider the gelatinous matter as the muscular system of this animal (*Abhand. uber Pflanz-th. Sprengel's edit.* p. 124—6), after he had repeatedly tried in vain to excite it to contract. One naturalist, well acquainted with the characters and habits of these animals, infers from analogy, that they possess nerves (*Phil. of Zool.* vol. i. p. 45); while another, who has likewise studied them in the living and dried state, maintains, that they are animals which possess no organ whatever, either for growth or generation (*Lamouroux Hist. de Polyp.* p. 14). From observing the canals of the sponge constantly empty, or filled only with water, Lichtenstein was led to believe this substance to be merely a dead mass of the empty tubes of alcyonia, remaining after the decayed polypi had been washed out (*Skriv. of Nat. Sel. Kiob.* 1794.). Blumenbach, and some other naturalists, apparently not aware of the close similarity of the fibrous axis of the sponge to that of some zoophytes, already known to possess polypi, and its dissimilarity to that of any known plant, and obviously not acquainted with the rapid currents and feculent discharges from its orifices, described by Ellis, Schweigger, Bell, &c. still regard the sponge as a plant, and consequently destitute of nerves, and muscular system, and polypi, and every kind of spontaneous motion (*Blum. Nat. Hist.* 1825.) This singular discordance of opinion among eminent naturalists of the present day, shows how little is yet known of the living organization and functions of this zoophyte, and the interesting field of discovery which lies open to those who love nature, and frequent the shores of the ocean.

In all the sponges I have met with alive, a distinct, soft, transparent matter, can be observed between the fibres; in some species, as the *S. panicea*, this matter is abundant and ropy; in others, as the *S. papillaris* and *coalita*, it is much thinner; and in others, as the *compressa* and *oculata*, it is found in smaller quantity. Probably no organized body can exist without similar soft parts. The fibrous part being always insoluble in water, can easily be procured separate from the soft matter, by immersing it repeatedly in hot water; it forms a net-work through every part of the body, and constitutes the *axis* or skeleton of this zoophyte, serving, as in other animals, to give form to the body, and support and protection to the softer organs. The axis is the part employed in the arts, or preserved in the cabinets of naturalists; it is the part of the animal which remains in a fossil state in the earth, as in the numerous fossil species found near Caen in France, (*Lamx. Exp. Meth.*); and it is that from which Aristotle and his successors have constantly taken the charters of the species. The structure of this part, or indeed of any other part of the sponge, cannot be observed without the assistance of the microscope; and it is well known that most zoophytes were regarded as plants, till the microscope reformed this part of science. But the minutest microscopical examination of the dried skeleton will not suffice alone to explain the living functions, or establish the nature of this animal. Lamarck, however, appears to have been misled by dried specimens or plates, or by preconceived hypothesis, in placing among the species of alcyonium the *Spongia cristata*, *S. tomentosa* or *urens*, *S. panicea*, and *S. palmata* of Ellis, which are common and well marked sponges, inhabiting our own coasts; and the *Spongia clavata* of Esper, which he has ranked as a variety of the *Alcyonium distortum*, has been lately shown by Schweigger to be a species of sponge resembling in texture the *S. oculata* (*Beob.* p. 29.)

The axis differs so entirely in its nature in different sponges, that the living properties observed in one species, ought with very great caution to be extended to any other, and naturalists may probably take advantage of this difference, in classifying or subdividing this numerous and obscure tribe. In some species as the *S. communis, usitatissima*, *lacinulosa*, *fulva*, *fistulosa*, the axis consists only of cylindrical tubular *horny* fibres, which dissolve without effervescence in acids, leave no trace when rubbed on glass, and consume like hair when burnt, emitting the same horny odour. In others, as the *S. compressa*, *nivea*, (a small sessile species with triradiate, quadriradiate, and simple spicula, to be noticed hereafter, which I have so named from its beautiful white colour), *botryoides*, *coronata*, *pulverulenta*, the skeleton consists entirely of calcareous spicula, which disappear before the blowpipe, do not scratch glass, and dissolve with effervescence in nitric, sulphuric, and muriatic acids. And in others as the *S. cristata, papillaris, tomentosa, panicea, coalita, oculata, dichotoma, stuposa, alcicornis, compacta, fruticosa, parasitica, hersuta, palmata, infundibuliformis, ventilabrum, hispida, suberica, nodosa,* we observe neither the horny tubular fibres of the first variety, nor the calcareous spicula of the second, but their whole axis is composed of minute *siliceous* tubular spicula, which, in dried specimens, appear drawn together into a longitudinal direction by the hardening of their connecting matter; these spicula scratch glass, do not dissolve in the above acids, nor consume by the blowpipe. The sileceous species abound on our shores, the calcareous are more rare, and I am not aware that any of the horny sponges has ever been observed so far north as the British shores.

Every one is familiar with the softness and remarkable elasticity of the common sponge, *S. communis*, which is the best example of the *horny* kind of axis. When a piece of it is brought near the flame of a candle, its fibres coil up, melt, and consume to a very small, light ash, with a horney smell, like hair; when a portion of it, well washed from sandy particles, is rubbed with a wooden instrument on glass, it leaves no perceptible streaks; when thrown into sulphuric or nitric acid, it diminishes in size, softens, and dissolves, without effervescence, into a brown pulpy matter, like other horny substances, and no spiculum is observed in the dissolved matter or precipitated to the bottom. Its fibres, and every thing of this nature, are best examined through the microscope, when they are suspended in water, and viewed by transmitted light. In this manner we observe them to be regularly cylindrical, translucent, of a brownish yellow colour, smooth on their external surface, all nearly of the same diameter, and distinctly tubular; they are tough, flexible, very elastic, generally quite straight, and they anastomose freely and completely with each other, through the whole body of the animal. Their diameter is about a third of that of a human hair, their length between their points of union varies from a tenth of a line to a line, and their internal tubular cavity occupies about half of their diameter, so that these horny fibres have a close resemblance to the spicula of many other sponges. From the clearness of the light transmitted through their central part; their internal cavity appears to be empty, which is not the case in the *S. fulva* and *fistulosa.* They unite at all angles, and they are a little dilated at their points of union; their internal cavities open freely into each other, and a small angular reservoir is formed at the place where they meet; they have no intervening connecting matter, no line of separation can be discovered at the angles where they pass into each other, and no opening is perceptible leading from their surface into their internal cavities; so that there is a continuous shut cavity in the interior of the fibres throughout the body of the largest common sponge, and these horny tubes winding round the pores and canals, cannot, therefore, be the cells of any kind of polypi, destined to create currents or other motions within the canals of this animal. The fibres unite so as to form polygons, whose sides lie almost always in different planes. The great elasticity of the axis shews that the orifices and canals, so obvious in this species, could not have been formed and left permanent, by any marine worms or insects merely traversing its texture; but must have formed a part of its original structure. The internal cavity of the strong horney fibres of the *S. fistulosa* and *S. fulva*, is completely filled with a dark granular opaque matter, which is continued from one fibre into another. This opaque matter renders the limits of the tubular cavity very distinct, and probably is the cause of these fibres being so remarkably hard and brittle, compared with the empty tubular fibres of the *S. communis*. The fibres of this last species, when highly magnified, resemble the empty stems of dead sertulariæ, from whose central axis the granular matter has been washed out, or consumed by animalcules, while the fibres of the two former species resemble the stems of living sertulariæ, whose central cavity is always filled with soft, moving granular bodies.

ART. I.—*Essay on Earthenware and Porcelain.*—[*Parkes's Chemical Essays.*]

[Concluded from page 481.]

"ALL the common ware undergoes two firings: that is, when it has been sufficiently dried in the air, it is put into an oven and made into what is called *biscuit*, which is earthenware partially burnt.

Many ladies are in the habit of buying china in the state of biscuit, for the purposes of painting and gilding. When they have imparted their own designs, these are sometimes returned to the potter, who burnishes the gold and completes the baking. This is mentioned with the view of directing those females who may not be already acquainted with it, to a new object for the exercise of their taste and genius, and also for the better explanation of the nature of biscuit ware.

All the English earthenware and china undergoes this previous firing, because, were it not first made into *biscuit*, it would not bear to be immersed in the mixture of lead and water, called the glaze, without suffering injury thereby: that is, it would not fail to be rendered soft, and liable to be put out of form, by its absorption of the water. Neither would it sustain the process of printing, nor allow of its being painted upon with any good effect.

The composition of the Chinese porcelain is different, and its nature is now pretty accurately known. Mr John Bradley Blake, who was settled as a resident supercargo at Canton, and died there at the age of 29, sent, a short time before his death, to Mr Samuel Moore, the then Secretary to the Society for the Promotion of the Arts, specimens of the earths, clays, sand, stones, and other materials used in making the true Nankin porcelain, all which Mr Moore put into the hands of the late Mr Wedgwood, who from these materials produced some pieces of excellent porcelain, and declared that those earths were so complete a set of specimens, that he had no doubt of their being the true materials of the Oriental porcelain. Had Mr Blake lived, further information would have been procured.

It was ascertained by Mr Blake, and we have learnt from other sources, that in China the earthenware and porcelain are merely dried in the air before glazing; as their composition so effectually resists water, that it can be immersed in an aqueous glaze, without being first made into biscuit, and consequently that the Chinese are enabled to burn their ware by one single firing.

It is therefore a desideratum with the potters of this country, to find some substance to mix with the clay which shall give it the property of resisting the action of water before it has undergone the first burning. This would be a great acquisition to our English manufactures, because it would not only be a saving of fuel, but a great economy of time and of the expense of wages, as one operation would be thereby entirely saved.

Dr Woodward mentions the Soap Rock at the Lizard

Point, and the steatites in other places, as very likely to make good porcelain.* Dr Campbell also, who seems to understand this subject, strongly recommends this article to be employed in the manufacture of china.† Whether steatites will improve the body of the ware, as above mentioned, I do not know, but I know it has been used at some of our porcelain works in considerable quantities.

Should the English potter be able to make the body of his ware equal to that of the East, it would then be necessary, as I apprehend, to discover some other material for the glaze; because, if our ware were burnt, as it is in China, by one operation, the earth would contract more than the glaze, and consequently the latter must either crack, or partially fly off.

I hazard this opinion, because I know that in China the glaze is infinitely better than ours, inasmuch as it consists purely of fel-spar, and is so extremely hard that it cannot be operated upon by any of our common cutting instruments. Even the wheel of the glass-grinder will not touch it; whereas ours becomes scratched and defaced by common usage and ordinary wear.‡

If the glaze on porcelain be too thick, this will occasion it to break; it is therefore important to apply a thin glaze, the fusibility of which should be as nearly as possible approaching to that of the body of the ware, in order that the combination may be more intimate and lasting. Chaptal describes a glaze which perhaps our potters may imitate with advantage. "It consists," says he, "in mixing the earth of Murviel in water, and dipping the pottery therein. When dry, they are plunged into a second water, in which levigated green glass is mixed. This covering of vitreous powder fuses with the clay of Murviel; and the result is a very smooth, very white, and very cheap glazing.§

The great durability of the Oriental china is its main excellence. We make porcelain in this country which is equally beautiful; and, if the symmetry of the figures and the merit of the painting be considered, far more elegant, although it possesses a less degree of tenacity and hardness.[*] We have, indeed, lately noticed the introduction of what is called stone-china, which is made very thick and clumsy to imitate some of the best productions of the East: the glaze upon this, however, is as poor and soft as upon other English china.

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* Woodward's *History of Fossils*, vol. i, page 6.

† *Political Survey of Great Britain*, vol. ii. page 19.

‡ This remark is not applicable to all the European porcelain; for that made in Saxony, and in some other parts of Germany, is equal to the best Oriental china.

§ Chaptal's *Elements of Chemistry*, English Transl., vol. ii. page 88.

Is it not natural then to ask, Why do not our potters make use of a better glaze[†], especially as the county of Cornwall can furnish felspar in abundance and at a reasonable price?

The true answer to this question seems to be this; that our ovens are not capable of fusing a glaze similar to that which is employed in China.[‡] The heat of the potter's oven in that country must be tremendous. The materials could not be vitrified as we see them, with less heat than would fuse Cornish granite.

An ingenious mode of discovering when the glaze on china is not sufficiently hard, has been given by the late Mr Nicholson. It consists in dropping a small quantity of strong ink upon it, drying this before the fire, and then washing it. If the glaze be too soft, an indellible brown stain will remain upon it.[§]

It must here be remarked, that for those who choose to incur the expense, there is porcelain manufactured in some parts of this kingdom which is more durable than that commonly made in the Staffordshire potteries. Such was formerly the Bow china, and that also made at Chelsea;[‖] and I have been credibly informed, the china now manufactured at Worcester is nearly equal to the best Dresden,[¶] or to the true Nankin, in all those properties which give the most value to this production of art.

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[*] It is probable that the heaviest and most compact china will always prove to be the most durable. Dr Watson, in taking the specific gravity of several specimens of pottery, found that a cubic foot of cream-coloured ware weighed 1988 ounces avoirdupois; of flint ware, 2188 ounces; of Bristol stone ware, 2340 ounces; and of East Indian china, 2346 ounces.

[†] In the year 1803, Professor Proust, of Madrid, published some remarks on the glazes of pottery. See his paper in the *Journal de Physique*, under the title of "A Memoir on tinning Copper Vessels" Extracts from this production will be found in the *Philosophical Magazine*, vol. xxi. p. 313.

[‡] The fusibility of the earthy compounds depends upon their proportions. Lime and clay are separately infusible; but, in due proportions, the mixture is capable of forming a perfect glass.

[§] *Chemical Dictionary*, article *Pottery*, 8vo. London, 1808.

[‖] Campbell's *Political Survey of Great Britain*, vol. ii. page 18.

[¶] Dresden china was formerly in such high estimation in this country, that in the year 1763 a single service of it was sold by auction in London, at Mr Uhthoff's sale, for 115 pounds. *Gentleman's Mag.* vol. xxxiii. page 312.

It is worthy of observation, that while the king of Prussia was pursuing every possible method of advancing and extending the manufactory at Berlin,[*] the works at Worcester continued to improve year after year, without patronage, and that they were brought to their present perfection solely by the genius and enterprise of the proprietors.

Of late years a porcelain manufactory of considerable consequence has been established at Coalport in the county of Salop. Some peculiar kinds of China, both serviceable and beautiful, are produced at these works.

The common heat of the potter's kiln in England is about 60° of Wedgwood's pyrometer,[†] which is equal to 8877 degrees of Fahrenheit for the cheapest cream-coloured ware, and about 80° of Wedgwood for the Staffordshire porcelain; though I have reason to believe that the manufacturers of the best cream-caloured ware seldom allow the heat of the kiln to exceed 45° of Wedgwood's pyrometer. It must be recollected, however, that we fire twice, and that these heats refer to what are employed in converting the ware into what is called biscuit.

I am not myself acquainted with the respective temperatures at which the other porcelain manufacturers fire their ware; but Dr Henry has stated that the Worcester china vitrifies at 94, that the Chelsea porcelain is fired at 105, and the Derby at 112 of Wedgwood.

In the Staffordshire potteries the temperature of the gloss-oven is generally about 10° of Wedgwood, or 2377 degrees less of Fahrenheit than the heat of the biscuit oven. The reason of this is, that there may never be any danger of the ware, in this last operation, undergoing a higher degree of heat than it experienced in the first burning; it being absolutely necessary for the operator to be careful of this point, because, as clay will continue to contract by every addition of heat, it must follow, that should it ever be pushed beyond that degree which it sustained in the state of biscuit, the body of the ware would experience a greater contraction, and consequently every piece within the oven would either become crooked, or the glaze would be injured. The firing of the gloss-oven generally requires from 12 to 14 hours.

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[*] In order to encourage this manufactory, and make its productions more generally known, the King made presents of complete services of the Dresden porcelain to all the sovereign princes in Germany.

[†] For an account of the construction of Wedgwood's pyrometer the reader is referred to the 72d and several of the subsequent volumes of the *Philosophical Transactions*, or to Murray's *System of Chemistry*, vol. i. page 150. The pieces of baked earth which he employed for the pyrometers were analysed by Vauquelin, who states them to have consisted of silica 64, alumina 25, charcoal 6, oxide of iron 0·2, water 6·2. Nicholson's 4to. *Journal*, vol, iii. page 265.

The glazing for the common cream-coloured earthenware consists usually of certain portions of litharge and ground flints, mixed with as much water as will form them to the consistence of thin cream, which is found to be a state the most suitable for the immersion of the ware. One hundred pounds of litharge and 40 pounds of ground flints are the common proportions; but it is usual to take 80 pounds of Cornish granite to 100 pounds of litharge, whenever that is used instead of flints. Flint is the article which is employed in making glaze for the porcelain as well as for the coarse earthenware, it being nearly all silica. Klaproth found common flint to consist of, ignited silica 98 parts, lime 0·50, alumina 0·25, ignited oxide of iron 0·25 parts, volatile in the fire 1.*

For the finer kinds of earthenware and for porcelain, it is made with white lead, ground flint glass, ground silica, and common salt. Some manufacturers, however, add other materials; most of those who superintend this department imagining that they possess a valuable secret, and that in consequence of it their glaze is much better or cheaper than that of others.

In all manufactories it is found necessary to vary the composition of the glaze, according to the nature of the materials which form the body of the ware; and the former cannot be perfect unless it be capable of contracting and expanding by heat and cold, in the same propertion as the ware itself will contract or expand by change of temperature. The writer of the article *Porcelain* in the French Chemical Dictionary very properly remarks that "a glass which makes a fine glazing for one porcelain will make a very bad glazing for another sort of porcelain, will crack in many places, and have no lustre. The glazing must be appropriated to the density of the ware and the ingredients of its composition."

As the number of potters has increased very much within the last 50 years, the competition has become so great that every manufacturer is under the necessity of consulting economy, and of sometimes sacrificing the utility and goodness of the ware, for the sake either of appearances or of adopting the cheapest modes of manufacture that can be devised. To one of these causes, may be attributed the introduction of *bones* into the manufacture of porcelain. Of this article there is now so large a consumption in the Staffordshire potteries, that some of the great manufacturers have extensive rooms which are used solely for the reception of bones. Animal bones are composed of lime and phosphoric acid, in different proportions according to the age and species of the respective animals to which they belonged. These are either bought of the indigent bone-collectors in the country, and then burnt and ground by the potter, or they are purchased ready burnt and ground for use, directly from those who distil volatile alkali or spirit of hartshorn. It is, however, generally known to the china manufacturers, that this article is injurious to the texture of the ware: its use, therefore, cannot be justified on any account whatever.

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* Klaproth's *Analytical Essays*, vol. i. page 42.

I allow that ground bones have the effect of rendering the goods very white, and also that they produce a transparent appearance; but I doubt whether this be a real transparency, or only a deception. Thus far, however, one may venture to assert, that the modern English porcelain, which has the usual proportion of bones in its composition, is of much less specific gravity than it ought to be,* and is very apt to crack with hot water. As the base of animal bones is phosphate of lime, it might be worth while for the potter who is determined to use bones, to try whether the *native* phosphate of lime† can be useful in making porcelain, and if phosphate of alumina may also be employed in this manufacture.

In returning to the subject of glazing it will be proper to state that the common earthenware would be too porous, and many kinds of fluids would pass through it were it not defended by an artifical covering or permanent glaze. It became necessary therefore to contrive a composition which should vitrify and flow in the fire, so as to cover these materials of earth with an entire coating of perfect glass.

Such a glaze having been found in a mixture of oxide of lead* and silica,† as mentioned above, a certain proportion of each of these, ground to an impalpable powder, is thrown into a tub of water, and stirred therein till the mixed powder becomes suspended and uniformly dispersed throughout the fluid.

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* For the specific gravity of good porcelain, see Note, page 528.

† A memoir by Hassenfratz "On the native phosphate of lime" may be seen in the *Annales de Chimie*, tome, i. page 191. Likewise some "Remarks on phosphate of alumina and on a mountain of calcareous phosphate," by Proust, in the same volume, page 196.

The glaze having been thus prepared, it is applied to the ware in the following manner. A workman to whom this duty belongs, receives the goods from a boy, who delivers them to him one at a time; and as he receives each single piece, he dips it in the liquid, lets it drain for a moment, and then, placing it on a board standing beside him, he is ready to receive another piece, which he immerses in like manner.

It is in this way that the glaze is applied to all the various articles which are made of the common earthenware; and as it is only partially baked or in the state of biscuit, when it is immersed in the glaze, a sufficient quantity attaches itself to the surface, which, when it has been fused, will form over it a complete covering of glass. This mixture has great fusibility, though silica alone does not melt in the focus of the most powerful burning mirror. Mr Hare, however, fused it completely by submitting it to the flame of a stream of mixed oxygen and hydrogen gases, and found that it formed when cold a kind of enamel‡.

There are, however strong objections to this mode of glazing pottery. The men who work in it for a considerable time are apt to become paralytic, probably from the lead:§ and where acids, which have the power of dissolving the lead, are employed in preparing condiments for our food, such a glaze must be extremely unwholesome.

Aware of the pernicious effects of lead, when taken into the stomach, M. Fourmy, an ingenious manufacturer at Paris, has prepared water-coolers, in which he has employed lava for the glaze, instead of the deleterious metallic covering which is more commonly used for that purpose. Pumice-stone has also, as I understand, been used for covering earthenware, and with very good effect.

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* The French potters, instead of an oxide of lead, use the common galena or sulphuret of lead for this purpose.

† Silica was formerly known by the name of the *vitrifiable* earth, because mixed with an alkali it possesses the property of fusing into a transparent glass.

‡ *Philosophical Magazine*, vol. xiv. page 304.

§ Some directions for the prevention of this malady will be found in *The Chemical Catechism*, tenth edition, page 101.

Some of the French potters, at the suggestion of Chaptal, have substituted common flint glass for lead in their glazing, and have found it not only safer, but more economical. They grind broken flint glass to a fine powder; and when this is mixed with a due portion of clay and water, the goods are dipped into it, as our potters dip their ware.

The potters of Bristol and Lambeth glaze their stone ware with common salt. When the oven has acquired a certain temperature, the salt is cast in, and the vapours arising from it attach themselves to the goods, and completely cover them. The alkali of the salt probably combines with the silica of the ware, and forms a true glass upon their surfaces.

The English potters are however fond of using a glaze made with lead and silica, because, whatever the articles may be which are emplowed for this purpose, it is necessary to give them a heat sufficient to vitrify the materials and convert them into a perfect glass; and when the best articles are made use of in forming a glaze, the heat which is employed must be very considerable; whereas lead alone will readily fuse and form a glaze that may be finished at a very low heat indeed. This, however, if used alone, would make a glaze too soft, and so extremely fusible that it would be apt to run very much towards the lower parts of the ware, and leave the upper ones imperfectly covered.

Half a century ago our potters were so careless in their manner of applying the salt glaze to the stone wares of Staffordshire, that large importations were made from France and Holland, where the manufacture was better conducted; and had it not been for Mr Wedgwood's timely invention of the Queen's ware, it is probable that the best part of the trade would long ago have been lost to this country.

A variety is sometimes given to the glaze by mixing certain oxides of the metals, especially that of copper, or otherwise the filings of copper; but this coloured glaze is generally applied only partially, and merely for goods of inferior quality. The edges of common plates are sometimes coloured by this method.*

It is about sixty years since the plain cream-coloured ware of Mr Wedgwood was introduced. At that time this unadorned and simple article was thought sufficient for every purpose where porcelain was not employed; and the neatness and cleanliness of its appearance gave it such a decided preference that her late Majesty allowed the worthy inventor to call it *Queen's ware;* but, when a demand arose for more expensive services of pottery, recourse was had to the pencil. From that period a great number of artists have been constantly employed in painting earthenware as well as porcelain, and often great taste is displayed in these productions.

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* The beautiful black glaze, which is seen on a peculiar sort of ware made at Nottingham, is composed of 21 parts by weight of white lead, 5 of flints, and 3 of the oxide of manganese. Watson's *Chemical Essays*, vol. ii. page 271.

It must not however be imagined that painting on earthenware was a new invention. The paintings of the ancients of this kind are rare and valuable. The Marquis Marcello Venuti, in his description of the curiosities found in the ancient city of Heraclea, speaks of one upon *slate*, representing a muse crowned with laurel, with a musical instrument hanging on her shoulders, which was found in one of the subterraneous cavities, and is described as being then in the possession of Signor Nicolo Vagnucci of Cortona, one of the principal supporters of the Academy of Tuscany.*

Nevertheless, it must not be concealed that Chaptal has shown, that the Romans of the first century were not acquainted with any of the metallic fluxes with which the moderns fix and vitrify the coverings of their pottery. For the particulars consult his very instructive account of certain colours which were found in the shop of a colour-merchant, in one of the streets of the ancient Pompeia, especially as he has there told us what was the composition of the glaze which the potters of that day did actually employ.†

To return to the consideration of the paintings on English earthenware, it must be observed that the variety of effect is produced by means of the different metallic oxides, each of which affords a different colour, and these colours are again multiplied by such mixtures of two or more of the oxidized metals, as experience has shown to be useful.

In employing these various colours, the ground oxide is first mixed with a prepared flux, which is also reduced to an impalpable powder, and then this mixture is well incorporated with gum water, the acid of tar, oil of turpentine, or some other essential oil, as may be most suitable, in point of expense or otherwise, for the goods on which it is to be employed. The fluids are used merely to lay on the colour;

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* Skurray's *Translation*, 8vo. London, 1750, page 107.

† *Annales de Chimie*, tome lxx, p. 22.

for it is necessary that, whatever oil be employed, it should have the property of evaporating entirely.

The preparations which are commonly used, are the metallic oxides and their combinations with acids. Thus cobalt yields a blue; antimony and silver give yellows and oranges; platinum a silver colour;* gold violet and purple; copper the greens; while the reds, the browns, and the blacks, are derived from iron.

The oxide of cobalt employed in the pottery is usually prepared from zaffre, which is an expensive article imported from Saxony, though a few years ago very fine oxide of cobalt was procured from Cornwall; for preparing antimonial yellows, the crude antimony is first calcined with four times its weight of nitre, and is then mixed with a certain proportion of vitrified lead. The precipitate of Cassius is the article which is generally used for the production of violets and purples, though sometimes the oxide of gold precipitated by copper is employed. For greens, the copper has usually been taken in the state of a precipitate; but some potters have found an article of more value in the pure oxide of copper, which they procure by placing sheets of copper in the ovens in which the ware is glazed; and it has lately been discovered that a small portion of copper mixed with the iron very much increases the intensity of the blacks on earthenware. Besides, iron itself is capable of giving a great variety of colours, according to the way in which it is managed. For instance, the black oxide of this metal produced by the action of heated air only, will be a very different article when used as a pigment from an oxide of iron prepared by other means.

It is here necessary to remark, that in painting on the biscuit no oil is used; the metallic oxides are mixed with water only; and it is owing to this circumstance that such ware may be glazed at once without being put into an oven, as is the case with those goods which are known by the appellation of *blue-printed.*

For certain purposes, however, the painting is performed *upon* the glaze, as some colours would be injured, and others destroyed, by the heat of the gloss-oven. Thus, where iron is employed to produce blacks or browns, the painting is always done *upon* the glaze; the ware is then finished in an enamel-oven, at about 6° of Wedgwood's pyrometer.

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‡ Some observations by Klaproth on the uses of platinum in pottery, may be seen in the *Phil. Mag.* vol. xvii. page 186.

Some metallic oxides are used for another intention besides that of painting, viz. for the purpose of covering the ware entirely on the exterior, as is the case when the sulphuret of antimony is employed for producing a common kind of yellow ware; and also for those articles known by the names of GOLD and SILVER LUSTRE. The first of these lustres is effected by means of gold, the latter by the oxide of platinum. For gilding porcelain the metal is used in its metallic state. To procure it in a pulverized form, it is dissolved in aqua regia, and then the acid is driven off by heat, and the gold remains; this is then mixed with borax and gum-water, which form together a proper vehicle for fixing it upon the goods. After this it is baked, and finished by burnishing.

For preparing the lustre ware, as it is called, the oxide, of whatever kind it may be, is mixed with some one of the essential oils, and in that state it is brushed upon the surface of the articles. It should have been observed, that when the manufacturer intends to make lustre-ware, the articles are glazed *before* the gold or the platinum is applied. For *gold*-lustre, the ware is made of a red clay, which, when burnt and glazed, shows so much of its colour through the covering of gold, as is sufficient to give the goods that peculiar brown tint which is always observable on this singular kind of pottery.

When the metallic oxide has been properly applied to the surface of the goods, they are carried to an enamelling-oven, where the heat dissipates the oxygen, and restores these precious metals to their metallic state,—I cannot say quite to their primitive metallic brilliancy, because this is often much injured by the fluid menstrum employed in the operation. The great difference which there is in the appearance of this ware, especially in that which is covered with platinum, can only be thus accounted for. Some of it looks like silver, while the articles from other manufactories are more like iron or steel.

I once imagined that this variation might arise from a different mode of preparing the oxide of platinum. I therefore prepared two samples of this oxide, the one precipitated by caustic ammonia, and the other by muriate of ammonia; but, when I had them tried by an experienced potter, both produced precisely the same colour.

I wish some manufacturer would attempt to produce this ware by mixing the oxide of platinum with pure water only; for in that case I am persuaded the ware when finished would be as white as the platinum itself.

The potters of England have derived great advantage from the introduction of the printing-press. The use of this valuable machine, which is comparatively of late date, has enabled these manufacturers to produce a greater variety of patterns, and of neater execution, than could possibly be acquired at a small expense by the pencil.

As this is a curious branch of the business, it will be proper to describe it a little more particularly. It consists in first printing the intended pattern with some metallic colour, chiefly the oxide of cobalt, on what is called *silver* paper, and then in transferring the colour from the paper to the surface of the porcelain. This style of colouring earthenware is a very successful imitation of the old blue porcelain of China, and of late years has been the means of extending the consumption of British pottery throughout Europe more than any other improvement in the manufactory; for the potters of China are totally unacquainted with the printing-press, and consequently all their designs are produced by the pencil alone.

This mode of imparting designs to the surface of earthenware or porcelain, and which is known in the trade by the appellation of BLUE PRINTING, is managed somewhat in the following manner.

One man constantly attends the press, which is very similar to our common copper-plate printing-press; and as soon as he has applied the colour, which is laid on the copper in the same manner as the copper-plate printers apply the ink, he lays it upon a hot iron, to thin the oil with which the colour is always mixed for this purpose. The oil which is used is a peculiar preparation of boiled linseed oil. When the colour upon the copper-plate is thus reduced to a proper consistence, a sheet of silver paper is laid over it, and the workman passes it, with the paper, through the press.

For blue printing, the oxide of cobalt is the only mineral which is employed. This is largely prepared in the Staffordshire potteries, and sells from 40s. to 60s. the pound, according to its intensity and goodness. Indeed, such improvements have been made in the manufacture of this colour, that the Chinese potters are now supplied from England with all the cobalt they consume.

When the paper comes from the printing-press, it is, of course, found to be stamped with the intended pattern. It is then delivered, while wet with the colour, to a girl, who cuts off the superfluous paper with a pair of scissars, and passes it to another girl, who immediately applies it to a piece of biscuit ware, and then delivers it to a third, who fixes it more firmly by rubbing it very hard with a piece of flannel tightly rolled up in the form of a short cylinder.

The design of this hard rubbing is to force the colour into the pores of the ware. When the papers which have been thus applied have lain on for about an hour, the colour is generally found to be sufficiently fixed to admit of their being detached. This is effected by putting the articles into a tub of water, where the paper soon becomes soft and pulpy enough to allow of its being peeled off by gentle friction, leaving the full impression of the pattern upon the biscuit.

The papers having been removed, the ware is suffered to stand a sufficient time to become dry, and then it is put into an oven at a low heat, for the purpose of dissipating the oil, and preparing it for receiving the glaze.

It must be obvious that it is necessary to employ a glaze which is transparent, in order to give full effect to the brilliancy of the cobalt colour. A little of this blue oxide is also generally mixed with the glaze, for the same purpose as laundresses employ smalt with their starch, viz. to increase the whiteness, by subduing any yellow tint that might otherwise impair its lustre.

Here I cannot avoid observing, that it would be an important acquisition if some suitable article, instead of *oil*, could be discovered for mixing with the colours which are employed in printing on earthenware, and which could be of such a nature as not to require to be burnt off previously to the application of the glaze. It appears to me to be worth while to institute a series of experiments for this particular object; because, if it could be attained, what immense quantities of printed goods, besides those which are ornamented by the pencil, might be completely finished with two firings, instead of three, which on the present plan, such goods always require! Were it on no other account than the expense of this vehicle, it is desirable to find a substitute. Some of the first enamel-painters think it necessary to use even the best rectified oil of amber, and this, of late years, has been very costly.

Another idea occurs to me respecting printed ware, which, however fanciful it may now appear, will, I doubt not, some time or other be realized. What I refer to is, the possibility of printing two or three different colours at once, like the Lancashire calico-printers. This, I presume, would be a discovery of great importance to every manufacturer of fine earthenware or porcelain.

A red oxide of iron more brilliant than any hitherto employed would be also a valuable acquisition. That which is commonly used is procured from green vitriol by calcination; but I would suggest that a much finer colour might be prepared from *nitrate* of iron, and advise the potter to take several portions of it and precipitate the iron by the different alkalies, each in a state of causticity; and by these means he would readily discover which produced the best and the brightest colour. If four distinct precipitates were made, viz. one with lime, another with potash, a third with soda, and a fourth with ammonia, it is probable they would be found to be of very different value for the purpose of painting on china. The alkaline carbonates would probably give precipitates of inferior qualities; but by a subsequent calcination the carbonic acid would be separated, and something valuable might be produced.

In making this experiment, it will be necessary to prove all these different oxides on exactly the same kind of porcelain, or the result might be deceptive; for it is well known to practical men, that the oxide of iron will not produce the same fine red upon *common* china, as it will upon the hard porcelain which is glazed with felspar.

The residuum which is produced by those makers of aquafortis who use sulphate of iron instead of sulphuric acid, might answer a good purpose, if thoroughly freed from the alkaline salt which it always contains.

It was my intention to have offered some remarks on the manufacture of crucibles, retorts, and other chemical vessels, which are required to sustain a great heat; but having already extended this Essay much beyond its proposed limits, I must content myself with merely referring to those authors who have already treated on this subject. The most important of these is Pott, who wrote a work expressly upon it,* as I have already mentioned.†

I shall detain the reader, however, just to notice that the chief difficulty which the manufacturer finds in preparing such utensils as crucibles, &c. is, that if the portion of silica were employed which is necessary to give sufficient compactness, the ware would, from this very circumstance, be liable to become useless; because many of our chemical articles have the property of dissolving silica. An expedient has therefore been adopted of mixing a large portion of old pottery with the fresh clay, which gives it the desired weight, and yet is not liable to be dissolved by the alkalies like pure flint or sand.

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* *Lithogeognosie Pyrotechnique, ou Examen Chymique des Pierres et des Terres.* 12mo. 2 vols. Paris. 1753.

† See Nicholson's *Chemical Dictionary*, 8vo. article *Pottery*; or *The French Chemical Dictionary*, under the same article.

I am moreover desirous of suggesting, whether it might not be advisable to mix alumina and silica in certain proportions; and, having kneaded this mixture into balls, to burn them in an oven at the highest temperature the mixture can endure; and whether such a preparation would not be more suitable for grinding with fresh clay, than the old pots which have hitherto been used for this purpose. I imagine that vessels made in this way would stand heat and cold better, because the pores of the old stuff are often so close, that it cannot expand and contract by alternations of temperature as it ought to do. I mean, however, very soon to ascertain this by some direct and decisive experiments.

I am informed by a Sheffield manufacturer, that their cast-steel makers mix ground coke with the best Stourbridge clay, and that their crucibles for melting steel are made with this mixture. Pure earthenware is not fit for crucibles, or for any vessels that must expand or contract by change of temperature. This has been long known; Dryden notices it in one of his poems.

It is said that in France the very best crucibles which they have, are made with fine clay mixed up with old ground butter pots; and that these are composed of a peculiar kind of stone ware, which is manufactured in Normandy.*

Where crucibles are required for melting metals, those made with plumbago are decidedly the best; but these are unsuitable for other purposes of the laboratory, especially when salts and alkaline fluxes are employed in any process.

In a paper by Vandermonde, Monge, and Berthollet on the manufacture of steel, and which was published by order of the Committee of Public Safety in Paris in the second year of the republic, they say, "One of the greatest difficulties we find in this country (France) is to procure good crucibles.* The art of pottery in a manufacturing country like Great Britain is in all its parts truly important.

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* See the article *Pottery*, in the second vol. of *The French Chemical Dictionary*.

More than eighty years ago the celebrated Reaumur, in making some trials on glass, discovered, that if a vessel made of the common green bottle-glass be submitted to the heat of a potter's oven, imbedded in a mixture of sand and ground *sulphate of lime*, the glass of which it is composed will be changed in its appearance, and acquire some of the properties of the best Oriental china; become so hard as to strike fire with steel; be semi-transparent like porcelain, and will endure sudden changes of temperature without breaking. An account of these results was given in the Memoirs of the Royal Academy of Sciences for the year 1739.

Having mentioned this, I have only room to add, that the late Dr Lewis, who conceived highly of this discovery, made many experiments to ascertain its nature and importance, the particulars of which, together with several most judicious remarks upon them, will be found in his celebrated work on the commerce of the arts.†

The subject, however, appears to me to be highly deserving of still further investigation, especially as useful vessels of a difficult form may be made more easily of glass than of earthenware. If, therefore, a cheap and certain method of performing this species of cementation could be adopted, the operative chemist would have it in his power to procure a greater variety of chemical vessels, and those not only more durable but of more convenient forms than he can possibly obtain by any other means. I wish some potter would commence a manufactory of this kind. Such ware would be highly useful for many domestic purposes, to which none of the common pottery can be applied either with convenience or safety.

In returning to the subject of earthenware, it is proper I should state my opinion, that chemical vessels are often injured by the oxide of iron originally pertaining to the clay itself, as it acts like a flux, rendering this earth too fusible to admit of a proper union with the siliceous material of the ground pottery, which is employed in the manufacture of such articles. A question arises here, whether *oxide* of iron alone is prejudicial in this way, although its sulphuret, the pyrites, may be. It may, perhaps, be worth while to decide this by experiment. Macquer says, that if a bit of pyrites be left in clay which is to be exposed to a baking fire, it will form a cavity in the ware, and that this may be known by the circumstance of the cavity being stained black.

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* An abridgement of this memoir was afterwards published in the 19th vol. of the *Annales de Chimie*, page 13—46.

† *Commercium Philosophico-Technicum*, p. 230—255.

There is an old pamphlet of 9 pages in the London Institution, without title-page or date, on making porcelain, in which the writer speaks of a practice the Chinese have of purifying the earth they employ for the glaze, by means of aqua-fortis. On reading this, it occurred to me, that it might perhaps be worth trying, whether this expedient could be adopted with advantage in purifying such of our clays as are designed for the finest purposes.

For those articles in which the chemical nature of the body of the ware is of importance, it would perhaps be adviseable always to wash the clay thoroughly; for, if pyrites be contained in it, the greater part of this would subside, on account of its superior specific gravity, while the argillaceous earth remained suspended by the water. For chemical jars, still heads, &c. a *small* portion of iron would probably not be injurious. Vauquelin analysed the clay of which Hessian crucibles are made, and found it to consist of 8 parts oxide of iron, combined with 69 of silica, 22 alumna, and 1 carbon.

Clay which contains lime is equally improper for the manufacture of chemical vessels. If a clay be suspected to contain lime, or rather carbonate of lime, it may readily be discovered by an acid, which, if this earth be present, will occasion an effervescence. When lime does not exceed five or six per cent., it appears to do no harm to the quality of the pottery; but when more abundant, it communicates too great a degree of fusibility: in like manner there are cases in which an admixture of the oxide of iron is useful and even necessary.

In the manufacture of what is called Delfware it has been customary to mix a portion of ferruginous clay with the other ingredients, because of its possessing a binding quality from which other species are exempt. The following are the usual proportions: two parts of red clay, three of blue clay, and five of marle.* Most of the red clay is indebted to iron for the colour which it assumes; but when iron enters in any considerable quantity into the composition of the finer kinds of earthenware, it imparts either a red or a brown tinge to it; and besides this, it adds more fusibility to it than even lime.

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* See Macquer's *Chemical Dictionary*, article *Delfware.*

Recollecting the circumstance just mentioned respecting delfware, it occurred to me, in reading one of the volumes of the *Archæologia*, that a certain iron ore found in Scotland would probably be useful, when mixed in a small proportion, for some articles of pottery. I will give an abstract of the passage which caught my attention; and if any ingenious manufacturer should be able to avail himself of this hint, and convert it to profit, I shall be rejoiced, and shall at any time be glad to be informed of the result, and the respective particulars which attended it.

In the Highlands of Scotland some very singular ancient walls are to be seen, which consist of "stones piled rudely upon one another, and firmly cemented together by a matter which has been vitrified by means of fire, and which forms a kind of artificial rock that resists the vicissitude of the weather, better than any artificial cement that has ever yet been discovered."

Mr James Anderson, the person who has written upon these curious remains of antiquity, has found that, through all the northern parts of Scotland, a peculiar kind of iron ore of a very vitrescible nature is in great abundance. Hence he supposes that "when the walls were reared and made as firm as could be by dry stones piled one upon another, the interstices between them were filled with this vitrescible iron ore, and the whole supported by a backing of loose stones piled behind it. When this wall was thus far completed, nothing more was necessary to give it the entire finishing, but to kindle a fire all round it, sufficiently intense to melt the vitrescible ore, and thus to cement the whole into one coherent mass.*"

Dr Lewis has related a remarkable instance of the affinity of iron for silica. Having immersed some pieces of green glass in the red oxide of iron which remains after the acid has been expelled from green copperas, and baked it for several hours in the upper chamber of a wind furnace, the glass and oxide of iron were found to have run together into a black mass, of sufficient hardness to strike fire freely with steel. "It is remarkable," says he, "that a metallic substance so refractory in the fire, should be so greatly disposed to melt with green glass."*

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* *Archæologia*, vol. v. page 255, where also may be seen an account of a fortification of this kind in the county of Ross, with a print of it, accompanied by some ingenious observations worthy of attention.

Nothing can be of more importance to a potter than a thorough knowledge of the properties of the different kinds of clays. Dr Campbell, in his Political Survey of Great Britain, states that we have not less than twenty-two distinct species;† and Macquer, who examined more than eight hundred specimens, says that in all that number he did not find one that was entirely free from metallic matter.‡

There is only one method, however, by which clays can be examined with any accuracy, and that is by the aid of chemistry. Such of my readers who are potters but not chemists, will therefore allow me to advise them to lose no time in acquiring the knowledge of analysing earths and minerals. Ample directions for the attainment of this art will be found in Kirwan's Elements of Mineralogy,§ or in Klaproth's Analytical Essays;‖ and I conceive it impossible that any manufacturer will ever regret the time which it may be necessary for him to devote to the acquisition of that elementary knowledge which would enable him to consult these distinguished writers with pleasure and advantage.

One or two instances of the mistakes which may occur in the examination of mineral substances, when they are not submitted to chemical analysis, will be sufficient to show the importance of this inquiry, and will, I hope, induce the junior practitioners at least, to adopt this advice, and enter upon that course of study which will be most likely to afford profit as well as entertainment.

The precious stones were formerly classed with siliceous fossils; and, until Bergman and Klaproth¶ analysed several of them, they were generally supposed to be entirely composed of silica combined with a portion of coloring matter, to which they were solely indebted for their distinctive characters and appearances. An accurate analysis of several of these gems, particularly of the Sapphire, the Topaz, the Hyacinth, and the Ruby, has, however, proved that either alumina, or zircone, and not silica, is in these instances the predominant ingredient. The Sapphire, according to Klaproth, consists of clay 98·60, oxide of iron 1, lime 0·50. The Topaz consists of clay 46, silica 39, lime 8, iron 6. The Hyacinth of zircone 70, silica 25, oxide of iron 0·50, loss 4·50.* The Ruby is composed of clay 40, silica 39, lime 9, and iron 10.†

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* *Commercium Philosophico-Technicum*, page 545.

† Campbell's *Political Survey of Great Britain*, vol. ii. page 16. Morton's *Natural History of Northamptonshire*, page 71.

‡ Macquer's *Memoir upon Clays*, presented to the French Academy in 1762, and abridged in the *French Chemical Dictionary*, under the article *Clay*

§ Printed in 2 vols. 8vo. London, 1794.

‖ Translated from the German, in 2 vols. 8vo. London, 1801.

¶ See Bergman's Essay, entitled, "A Dissertation on the Earth of Gems," in his Collection of Essays, vol. ii. p. 76—121; and Klaproth's *Analytical Essays* before mentioned.

The well-known article called Fuller's Earth, from its saponaceous texture had been supposed to consist almost entirely of clay or alumina; but on an accurate analysis, it has been ascertained that the earth which enters into its composition in largest proportion is silica. According to the analysis of Bergman, Fuller's Earth consists of silica 51·8, alumina 25, lime 3·3, magnesia 0·7, iron 0·7, water 15·5. Hence it may be accounted for, why it falls into a powder when put into water, and is incapable of forming a ductile paste. On the contrary, Emery, which has very much the appearance of sand or ground silica, is for the most part argillaceous earth or alumina, and the siliceous earth is in a very inconsiderable quantity.‡

The last instance which I shall adduce is one that is intimately connected with our subject, and which shows decisively how necessary it is for a potter to be capable of undertaking a chemical analysis.

The porcelain earth of Baudiffere was pronounced by Macquer and Baumé to be a CLAY superior in quality to that which was then employed in the manufactory belonging to the French King at Sèvres; and on their authority this was generally believed, till Mr Globert discovered that not a particle of clay could be found in it.§ For, when this chemist attempted to convert it into alum, he found, to his great surprise, nothing but very pure crystals of Epsom salt, or sulphate of magnesia. After this, Globert analysed this mineral with great care, and found it to consist of magnesia 68,

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[*] Klaproth's *Analytical Essays*, vol. i. p. 198.

[†] Bergman's *Essays*, vol. ii page 101.

[‡] Emery from the Isle of Naxos is composed of alumina 80, silica 3, and iron 4, undissolved 3 parts. Tennant's *Analysis, Phil. Trans.* for 1802, page 401.

[§] See Murray's *System of Chemistry*, vol. ii. page 253.

carbonic acid 12, silica 15·6, sulphate of lime 1·6, and water 3*.

Since that period magnesian earth has been much employed in the composition of porcelain, it having been ascertained that a small portion of it prevents that great degree of contraction which always takes place in the fire, when clay and silica alone are made use of. The Cornish steatites or soap rock being a mineral containing magnesia, some of the English potters now employ it for this purpose.

I have understood, that in England the Steatites was first used by the porcelain manufacturers at Worcester, and that they paid at the rate of £20. per ton for it. In a well-known publication it has been stated that much of the land is in possession of the great Staffordshire houses, who have taken leases of it for the purpose of possessing themselves of this very valuable natural production. I learnt, however, in the year 1815, from most respectable authority, that *steatites* had then never been employed by the Staffordshire potters.

The steatites of Cornwall occurs at the Lizard Point, in a serpentine mountain which it cuts through in small perpendicular veins, called *rake-veins*. The finest sort is white, with blueish or redish spots, resembling marble. The best analysis of this mineral gives silica 48, magnesia 20·50, alumina 14, oxide of iron 1, water 15·50, loss 1.†

It might perhaps be worth while for the glass-makers to try the effect of a small mixture of *steatites* with the materials of which they make their large crucibles; because, if it prevent that great degree of shrinking to which they are now liable, it would be very advantageous.

I have been informed by an eminent maker of plate glass, that their crucibles, which are at first 32 inches deep, will not measure more than 29 inches when they have undergone the heat of the oven and become thoroughly baked; and that this contraction has occasioned very great loss, especially in respect to the excise duty.

These glass-house pots are very large vessels, being made to measure nearly as much in diameter at the top, as in depth, consequently the shrinking on the whole is very great; and as the officer of excise gauges them before they are burnt, the manufacturer incurs a loss of duty upon as much glass as might be contained in the depth of 3 inches, and this loss recurs every time such crucible is charged; so that a manufacturer who pays £5000. per annum in duty, must pay £500. every year more than was contemplated by the legislature.

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* Nicholson's *Journal*, vol. xii. page 277, from the *Journal de Physique*, vol. lx.

† Klaproth's *Analytical Essays*, vol. i. page 464.

I am very glad however to have it in my power to inform my readers, that since this was written for the first edition of this work the hardship there complained of is entirely removed, the government having made an alteration in the mode of collecting the duty on glass; for, instead of gauging the crucibles, the duty is now charged on the finished glass itself according to its actual weight.

Having ventured to offer these observations, I shall conclude with a few hints which to practical men may perhaps be useful.

There is a clay found in the territory of Sienna, in the province of Tuscany, called *fossil meal*, and by Kirwan, Argillomurite, which is capable of making hard compact bricks that will float in water, either baked or unbaked. M. Fabroni, who examined some of these bricks, in consequence of their having been mentioned by Pliny,[*] says that they may be of great use in the construction of reverberatory furnaces; for they are such bad conductors of heat, that although one of such bricks should be made completely red hot from one end to its middle, any person may take it up in his hand by the other end with impunity.[†] May it not then be worth while to inquire, whether a clay possessed of similar properties cannot be found in Great Britain?

Moreover, considering the improvements which have been made in the manufacture of pottery, might it not be easy to make bricks to imitate stone and yet be as durable as the bricks now in use?

By means of the metallic oxides, would it not be possible at a cheap rate to stain bricks of any colour, and might not such bricks be made to imitate marble?

In the Essay on Barytes, vol. i. page 317, I have stated, that Mr Wedgwood employed the sulphate of that earth in the manufacture of jasper; but if pure barytes itself or strontites could be procured without much expense, might not these also be used with advantage in some of the branches of pottery? A method of employing sulphate of barytes in pottery is indeed alluded to by Sage, in one of the volumes of the *Journal de Physique*. "The Chinese," says he, "know the remarkable property which gum adracanth has of giving a coherence to minute stony substances, which are then unalterable by fire; and that if this gum be mixed with pounded sulphate of barytes, it may be formed into masses which neither alter their shape nor solidity from exposure to an intense fire.* I have not heard that any English potter has yet availed himself of this suggestion.

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[*] See Pliny's *Natural History*, lib. xxxv. chap. 14.

[†] See *Philosophical Magazine*, vol. ii. page 180.

The sulphates of barytes and strontites may be had cheap and in abundance; and all that would be necessary is to roast them in a reverberatory furnace with some combustible matter, to convert them into sulphurets, and then the remaining sulphur, should it prove injurious, could be separated by a subsequent process.† In this way an ingenious manufacturer might probably gain unexpected profits and improvements from one or both of these earths.

In concluding this Essay, I trust I shall stand excused, if I endeavour to confirm an opinion already given, that a potter should never employ a new porçelain-clay without previous analysis; for there are few of these clays that can be employed with the certainty of a good result, unless some fusible material be used with them; and the quantity of this can only be known by a chemical examination of the clay in question. The necessity of analysis will appear from the comparison of the chemical nature of three of these native productions. The porcelain earth of Limoges, and which is often used without any admixture, is, according to Hassenfratz, composed of 62 parts silica, 19 alumina, 12 magnesia, and 7 sulphate of barytes; whereas the porcelain-clay of Cornwall is a compound of 20 per cent. of silica and 60 per cent. of alumina; while that found in the department of the Loire in France, of a beautifully white colour, consists entirely of carbonate of magnesia and silica.

The business of a potter affords so large a field for the exercise of taste and genius, and the art itself is so intimately connected with chemical knowledge and experience, that it would be superflous to urge, by further arguments, the absolute necessity of cultivating the science of chemistry, in order to arrive at perfection in any branch of the manufacture.

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* See the *Retrospect of Philosophical Discoveries*, vol. i. p. 50.

† An account of a process by which this might be effected may be seen in Essay V. vol. i. page 361.

Paged wrong from here.

Art. LI.—*The Geological Deluge, as interpreted by Baron Cuvier and Professor Buckland, inconsistent with the Testimony of Moses and the Phenomena of Nature.* By the Rev. John Fleming, D. D., F. R. S. E.—[*Edin. Phil. Jour.*]

[Concluded from page 485.]

11. *Formation of Gravel Beds.*—The materials of which these beds consist, appear, in general, to be rounded blocks of rocks, confusedly mixed together, or presenting but indistinct marks of stratification. The blocks are seldom angular, and never exhibit the surfaces or edges of a mass recently detached from an undisintegrated rock. As these masses are supposed to have been derived from the rocks which the geological deluge tore from their beds during the excavation of the valleys, we might expect to find them exhibiting numerous instances of tolerably fresh fractured surface, and the edges and corners still nearly entire. But when we find the reverse of all this generally to be the case, we must draw the conclusion, that the force of the agent, which collected the contents of these beds, was chiefly expended on the loose and weathered blocks on the surface. This is a fact of some value, especially when viewed in connexion with other characters exhibited by the gravel.

The clay or loam associated with the gravel, according to Professor Buckland, "possesses no character by which it is easy to ascertain the source from which it has been derived, *but usually varies with the nature of the hills composing the adjacent districts.*"—(*Rel. Dil.* 191.) On the supposition that this loam was derived from the finer portions of the soil and detritus removed by the waters of the deluge, we might expect that it would possess something like a common character, not in England only, but over the globe. But when we see it vary with the nature of the neighbouring hills, and consequently with the soil and detritus which they produce, we are irresistibly led to infer the operation, not of a universal, but of a *local* agent.

According to Professor Buckland, the "diluvial gravel is almost always of a compound character, containing amongst *the detritus of each immediate neighbourhood, which usually forms its greatest bulk*, rolled fragments of rocks, whose native bed occurs only at great distances, and which must have been drifted thence at the time of the formation of the gravel, in which they are at present lodged."—(*Ib.*) The *rolled* character of the gravel is fatal to the supposition of a *sudden* and *transient* inundation, acting upon fresh portions of dislocated strata. The circumstance of some of the blocks having travelled from a distance, is equally satisfactorily explained, on the supposition of a partial flood, occasioned by the bursting of an alpine lake, as by a sudden and universal flood. We can scarcely, however, avoid asking the question, Would not a general flood, raging violently, have produced gravel, of so confused and mixed a character, as to render it difficult to trace the origin of its materials? This *local* character, though apparently hostile to the diluvian hypothesis, is of importance to society in an economical point of view. Norway has suffered much from this transient flood, for, according to Professor Buckland, pebbles of her rocks have been carried to England. But our country has been more highly favored. Had it been otherwise, instead of gold reposing at the base of the Leadhills, or stream tin in Cornwall, they had been resting far from their birth-place; probably, if the deluge was from the north, in the bottom of the Bay of Biscay.

There is one character exhibited by the boulders in the gravel, of a truly interesting kind, in a theoretical point of view,—the intervention of valleys between the rocks from whence they came and the station they now occupy. It seems to be admitted on all hands, that these valleys did not exist at the period of the transportation of the gravel. Mr Greenough declares, that "the blocks of granite on the Jura attest the non-existence of the Lake of Geneva at the time of their transportation,"—(*Geol.* 177.); and, according to Professor Buckland, "the quartzose pebbles found on the tops of the hills round Oxford and Henly, were drifted thither from the central parts of England, before the excavation of the present valley of the Thames."—(*Rel. Dil.* 248.) If, then, we consider the gravel as diluvian, the valleys must be regarded as *postdiluvian*; or, if we consider the valleys as having been formed at the deluge, then the beds of gravel must be regarded as *antediluvian*. Professor Buckland has endeavored to avoid the admission of these conclusions. "It seems probable that the *first rush* of these waters drifted in the pebbles within the great escarpment of the oolite, and strewed them over the then nearly continuous plains; and that the valleys were *subsequently* scooped and furrowed out by the *retiring action* of these same waters."—(*Rel. Dil.* 253.) Is it conceivable that this sudden, transient and impetuous deluge, should have transported, in its first rush, various kinds of boulders, ten, twenty, or hundreds of miles, strewed them over nearly continuous plains, and then proceeded to scoop and furrow out numerous, deep and extensive valleys in these plains, whilst it permitted the deposits of its first rush to retain undisturbed possession of the station to which they were first brought? Could I bring my mind to assent to such statements, I should claim to rank with Judæus Apella. But the difficulty does not end here. In these valleys, supposed to have been excavated by the *retiring* waters, extensive depositions of gravel occur. (*Rel. Dil.* p. 251—2.) This last circumstance, which is far from uncommon, marks a third epoch in the history of valleys and gravel. In the first period, the gravel was transported across continuous plains. In the second, valleys were scooped out. In the third, the bottom of these valleys received deposits of gravels. These facts intimate successive operations, executed under different circumstances, and seem fitted for leading to the inference, that some time intervened between the several changes. They certainly do not support the conclusion, that the three phenomena had their origin in the same sudden and transient inundation. Under all the circumstances of the case, the young geologist will feel himself without a guide, and without a test, in determining the æra of the formation of a bed of gravel. 1. It may be antediluvian, produced by the bursting of a lake (for lakes must have been numerous, indeed, and extensive, before the excavation of so many gorges and valleys by diluvial action), spreading its wreck on nearly continuous plains. 2. It may be the result of the first rush of the diluvial waters, previous to the formation of the valleys of denudation. 3. It may be the wreck of these valleys, produced during the tumult of the retiring waters. 4. It may be the result of the very last effort of the flood, to fill up the frightful excavations it had produced in the fury of its retreat. 5. It may be postdiluvian, and the result of the bursting of an alpine lake: and this gravel may have been deposited at very distant intervals. On the banks of Glenmornaalbin, diluvium may occur, referrible to four different burstings of the Lochabor lakes, and all of them prior to human record. The diluvium of Martigny, from the bursting of a lake, was formed in 1818. When all these probabilities are taken into consideration, few, who generalize with ordinary caution, will feel inclined to refer to one æra the formation of all our irregular beds of clay and gravel.

Independent of the depositions of confused portions of gravel and loam, there are likewise extensive depositions of sand,

and gravel, and clay, of the same materials as the so-called diluvium; but which, by being divided into beds and strata, indicate a subsidence from water in a state of comparative stillness. The characters of these beds seem to have been in a great measure overlooked by the advocates of the diluvian hypothesis. It is not probable that such beds could have been produced by a sudden and transient flood, which, in its first rush, transported "Norwegian pebbles" to the plains of England; and, by the impetuosity of its retiring waters, scooped out the Solway Frith, the English Channel, and the Lake of Geneva. On the other hand, a lake at a high level, bursting its barrier, and carrying the wreck into a lake at a lower level, would give origin to stratified gravel, sand, and clay, such, for example, as may be seen in the neighbourhood of Edinburgh, and on the south bank of the estuary of the Tay; and which lower lakes have in their turn been drained.

The last character which I shall notice belonging to those beds of loam and gravel supposed to have been formed by the deluge, is the presence, exclusively, of the remains of *land animals*. This fact is supported by the testimony of Professor Buckland, in his "Inaugural Lecture," and "Reliquiæ Diluvianæ;" by Mr Greenough in his "Geology;" and by Mr Conybeare in the "Geology of England and Wales." This character yields a demonstration, that the water, which in its fury produced or transported this gravel, passed over a portion of the Earth's surface, on which dwelt land animals, and that a flood from the sea had not been concerned in the phenomena in question. To the matter confusedly brought together by this flood or floods of *fresh water*, I have, in my second paper on the "Modern Strata," given the name of *Lacustrine Diluvium*. Had a sudden, universal, and transient deluge been the agent concerned in its formation, then should we have looked for the remains of the *animals of the sea*, mingled in sad disorder with those of the land and the lakes; or rather fishes, shells, and zoophytes, where we now find the wreck of land animals*. Even the peculiarities of the remains of the land animals stand opposed to the geological deluge as it has been interpreted; for these belonged to individuals, which, according to Professor Buckland, "lived and died in the regions where their remains are now found, and

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* In my first paper I had enumerated *five* characters of lacustrine diluvium, indicating, that a universal flood had no share in its formation. Four of these characters are admitted, directly or indirectly, in the "Reply." But the fifth ("the absence of marine exuviæ,") is brought forward against me as an example of "*misstated facts:*" and it is added, that if I had ever seen or heard of three examples, which are quoted, of the presence of marine remains, I never would have advanced such an argument. One of these examples is unfortunate, as the learned Professor seems to confound three

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different formations,—the crag, or *upper marine formation*; distinguished from those of the *modern epoch*, by the species of shells, but especially the zoophytes, which it contains:—the *Lacustrine Diluvium*, containing the remains of land animals:—and *Marine Diluvium*, containing the relics of existing marine shells of the neighbouring sea. In my second paper, I intimated my acquaintance with his two first examples, and I added six others, with which he might have been acquainted. Yet my opinion remains unchanged; and I misstate no facts, while I preserve a distinction in geology (which my opponent will soon find it necessary to adopt) between Lacustrine and Marine diluvium. In the appendix to his paper he recurs to the same subject, and considers, that the facts I advance in my second, are in "direct contradiction" to the opinions advanced in the first. Here he labors under *ignoratio elenchi*, which a reperusal of my two papers would readily remove. If I allow him to use *my terms* with *his different signification*, I have too much respect for his logical powers to anticipate a failure in his object. But if the terms I use be taken in the sense in which I have defined them, the charge of "contradiction" will be found without proof.

Professor Buckland, rising, as it were, in his demands, having fancied that I had contradicted myself, announces the cause of my misfortune and the extent of my guilt.—"*Not being aware* of facts which so materially affect his argument, at the time of his writing the paper in question; at any rate, it would have been more *candid to acknowledge his error*, than to leave to me the task of pointing it out, and applying it to my advantage in the matter at issue between us.' Is it *probable* that I could have been ignorant of *eight* facts at the time of writing my first paper, which I give in detail in the *continuation*, or second paper; or that I would record these eight facts in the second paper, which contradicted my stataments in the first, without offering any explanation? Low, indeed, must be my rank in the intellectual scale, in the opinion of my opponent, if he be disposed to reply in the affirmative. But I can produce evidence that it was not *possible* I could be ignorant of *some* of the facts at least, stated in my second at the time I wrote the first paper, nor for eighteen years previous. The first of the eight examples of marine diluvium in Scotland which I quote, is from a *published paper of my own*, and to which there is a particular reference, on a bed of sea-shells, on the south banks of the estuary of the Forth. This bed, *as is stated in my second paper*, I examined in 1806, read an account of it to the Wernerian Society in 1811, and published this account in the Annals of Philosophy for August 1814. I may even go farther, and say, that it is *not probable* that Professor Buckland was ignorant of this *demonstration* of my previous acquaintance with these reputed contradictory facts. He quotes Captain Laskey's paper on the marine shells of the Paisley Canal, from the Annals of Philosophy for February 1814, and my paper, referred to, appeared in the same work, in the number for August of the same year. The Wernerian Memoirs, which he also quotes, contain a similar reference. But the most convincing proof of all (on the supposition that he read the paper he attempted to criticise) is the fact of this example of marine diluvium being *the first* of the facts I adduce in illustration of the history of the formation. By quoting in the "Reply" only the *last* of my *eight* examples, the reader may be misled into the belief that the reproach is merited. Whereas, had the *first* of them been quoted, as justice required, it would have carried on its front the refutation of the charge of ignorance and want of candor, it has been somewhat hastily brought forward to support.

were not drifted thither by the diluvian waters from other latitudes. (*Rel. Dil.* 44.) It is impossible for me to form a conception of a sudden, violent, transient, and universal flood, which transported Norwegian pebbles to England, yet did not bring along with these a few carcases of the truly arctic animals, such as the white bear; neither floated off to Africa the land animals which were browsing on the continuous antediluvian plains of England. To me it is equally inconceivable, that the inhabitants of southern and tropical countries, were not drifted northwards, and a few of them left in England by the agency of the retiring waters. Yet our diluvium contains not the productions of the polar or equatorial regions, but exclusively the remains of the early inhabitants of the British soil. This character furnishes another demonstration, that the agent or agents concerned in producing the diluvium, must be regarded as having possessed only a limited or local authority. We must be careful here, not to confound with "Lacustrine Diluvium," deposits on which I have bestowed the title of "Marine Diluvium." Portions of this diluvium have been formed within the period of authentic history; other portions are of earlier origin. The bones of land animals may occasionally be expected to occur in this formation, as the inundations of the sea, by which it has been produced, might have mixed with the spoils of the deep the relics of the dead or living terrestrial inhabitants which it met with in its progress.

3. *Mud in Caves.*—In the celebrated cave at Kirkdale, there is a layer of mud in the bottom, inclosing the fossil bones, and over this bed there is a covering of calcareous stalagmite. Professor Buckland considers the bones to have been carried in by hyænas as their food, when they dwelt in this den anterior to the deluge; that the mud was introduced by the waters of the deluge; and that the stalagmite is decidedly postdiluvian. (*Rel. Dil.* 48.) Another explanation is offered by the same author, of the mud and bones which occur, nearly filling several caves in limestone rocks at Plymouth. Instead of having recourse to hyænas as carriers of the bones, he says, "that the animals had fallen during the antediluvian period into the open fissures, and there perishing, had remained undisturbed in the spot on which they died, till drifted forwards by the diluvian waters to their present place, in the lowest vaultings with which these fissures had communication." *Rel. Dil.* 78.

The safest way of proceeding, in such circumstances, is to endeavour to discover some analogous phenomena, the history of which is not involved in obscurity, and apply the explanation which offers itself in the last cases to the illustration of those which are more ancient and obscure. Fortunately such cases are accessible. In Wokey Hole, in the Mendip Hills, a cave occurs with lateral chambers; mud likewise occurs; and in this mud are found human bones, and a piece of a sepulchral urn. These *bones* are said to be "very old, but not antediluvian." Where is the proof? or how are we to distinguish between antediluvian and postdiluvian bones? The mud, too, is "evidently fluviatile, and not diluvian." How are we to distinguish between fluviatile and diluvian mud? Not by their contents, for bones are present in both. Not by a difference in juxtaposition, for both occur in caves with the floor as their bed, and stalagmite as a covering. The evidence, however, of the mud being fluviatile, may be considered as complete, as the spot on which it rests is within reach of the highest floods of the adjacent river. It may thus be assumed as a fact, that local inundations or floods are capable of conveying mud into caverns, and depositing it on their floors, under circumstances perfectly analogous to the so-called "diluvian mud," and of surrounding "postdiluvian bones" as the diluvian mud is supposed to have surrounded "antediluvian bones." In another cave in the same neighbourhood, numerous bones and skulls of foxes were found. It is likewise stated by Professor Buckland, that, at a little distance from the Cliff of Paveland, "is an open cavern, to which it is possible to descend only by a ladder, and which, like the open fissure at Duncombe Park, contains at its bottom, and in the course of its descent, the uncovered skeletons of sheep, dogs, foxes and other modern animals, that occasionally fall into it and perish." In reference to these natural pitfalls and accumulations of bones, the learned professor offers the following sensible observations: "Animals at this day do fall continually into the few fissures that are still open; and carnivorous, as well as graminivorous animals, lie in nearly entire skeletons in the open fissure at Duncombe Park, each in the spot on which it actually perished, upon the different ledges and landing places that occur in the course of its descent; and from which, if a second deluge were admitted to this fissure, it could only drift them downwards, and with them the loose angular fragments, amidst which they now lie, to the lowest chambers in which the bottom of this fissure terminates." (*Ib.* 78.) The *bones* in caves may have been drifted in from open fissures at a high level by water, whether in the character of a local or extended inundation; and the *mud* may be referred to a similar origin. But, in all this, there seems no ground to infer the exclusive agency of one sudden and transient deluge, when causes still exist, though of a more humble kind, adequate to produce the phenomena.

The cave of Kirkdale does not present any appearances, warranting an explanation different from that which applies to acknowledged postdiluvian fissures and caves. The rounded cavities in the bottom of the cave, resembling, according to Mr Young, "such water-worn hollows as we see in rocks, in the beds of rivers, or on the shores of the ocean," prove, that, at a period antecedent to the introduction of the bones, this was a fissure in the limestone traversed by a subterraneous river. This is rendered more than probable, by the numerous other fissures existing in the same bed, into one of which, in the immediate neighbourhood, the Rical Beck enters, and for a certain space becomes a subterranean river.* We have here, therefore, an agent capable of bringing in the mud and bones from higher fissures, if such existed, and depositing both in their present situation. The existence of such fissures cannot be doubted, since Professor Buckland has made the concession. "The fact already mentioned of the engulfment of the Rical Beck, and other adjacent rivers, as they cross the limestone, showing it to abound with many similar cavities to those at Kirkdale, renders it likely that other deposits of bones may hereafter be discovered in the same neighbourhood." But are there not open fissures in this bed of limestone still existing, as natural pitfalls for modern animals, and furnishing intimations of the former state of the district? "In Duncombe Park, in the immediate neighbourhood, and in the same limestone rock, there is at present an irregular crack or fissure twenty feet long, and three or four feet broad, which is almost concealed and overgrown with bushes, and which being nearly at right angles to the edge of the cliff, lies like a pitfall across the path of animals that pass that way. It descends obliquely downwards, and presents several ledges or landing places, and irregular lateral chambers, the floors of which are strewed over with angular fragments of limestone, fallen from the sides and roof, and with dislocated skeletons of animals that have, from time to time, fallen in from above and perished." (*Rel. Dil.* 55.) The fissure was found to "contain the skeletons of dogs, sheep, deer, goats and hogs." "The bones lay loose and naked." A local inundation flowing into the fissure would transport the bones to the lowest chambers, and leave them in the same circumstances as the so-called antediluvian bones. The evidence thus appears to be in favour of that opinion, which supposes that the bones in the Kirkdale cave were brought to their present situation from caverns at a high level, by the agency of water, which deposited at the same time the mud in which they are imbedded. I say imbedded, because the mud does not appear simply to have filled up the interstices or layers of bones, but to have *suspended* and enveloped many of them. "Most of them are broken into small angular fragments and chips, the greater part of which lay separately in the mud."—(*Rel. Dil.* 12.) The present existence of pitfalls, and subterranean rivers in the same limestone, likewise gives strong probability to the inference which we have drawn, or rather would amount to a proof, provided there be nothing in the condition of the bones themselves, justifying the propriety of another explanation.

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* The proof which is brought forward by Professor Buckland, that the Kirkdale Cave was not formed or modified by the agency of water, is singularly defective. The sides "are constantly *rough*." Were they never smooth? The limestone in which fossil shells are imbedded decays more rapidly than the relics it encloses, when exposed to the weather, or to damp air; as the surface of every secondary limestone testifies. (Take the columns of St Paul's as an example.) Nor is the proof, that the bones in the same cave could not be introduced by running water, more satisfactory; "because it is impossible that now, or at any past period of time, any river should ever have flowed there." A river flows, at this moment, not a hundred feet distant, and its channel is only 36 feet lower than the cave. There are many other rivers in the neighbourhood, which flow over the same bed of limestone, in which the cave is situate, and this rock is full of fissures. The reader, from these facts, will be able to estimate the value of a *geological impossibiliy*.

The bones in the caves are chiefly fragments, and besides the small splinters, numerous portions of the ends, or the most solid portions of the larger bones, the jaw and teeth, occur. Some of these splinters are angular, "but many others were decidedly rounded and smoothed at the projecting parts, bearing obvious marks of having been long agitated by water."—(Young, *Wern. Mem.* iv. 266.) These circumstances confirm the supposition, that the bones were drifted into their present position by water, especially when we keep in view, that the bones of the different species were found co-extensively distributed "even in the inmost and smallest recesses." (*Rel. Dil.* 16.) Professor Buckland, in endeavouring to establish his hypothesis, that hyænas dragged in the bones in question, considers the rounding of the fragments as having been produced by the treading of the animals in the bottom of their den. His chief argument, however, is derived from indentations which are observable on some of the bones, and which he refers to the nibbling of the hyænas while cracking the bones, in order to extract the marrow. Even admitting that these indentations have been produced by the teeth of hyænas (an opinion not rendered even probable), still we would adhere to the explanation already given, since these markings may have been produced by hyænas on the bones as they lay in the original pitfall, to which these depredators may have had access. In reference to the marks or pits on the ulna of a wolf and the tibia of a horse, occasioned, in the opinion of the Professor, by the canine teeth of an animal of the size of a weazel, he adds, "These pits must have been formed before the bone was imbedded in mud in the lowest recesses of the cave, and probably whilst it lay exposed in some upper cavity of the rock." Why refuse to adopt a similar explanation of the larger markings on the Kirkdale bones? But, if the hyænas carried in *all* the bones, it may be asked, why did they transport those of such small animals, as water-rats, weasels, rabbits, pigeons, snipes, and even larks, —animals, which, to a hungry hyæna, would not be a mouthful. But the difficulty increases when we consider, that, if the evidence is conclusive to prove that the hyæna carried in the bones of the elephant and rhinoceros, and reduced them to fragments, it equally proves that the small bones of these animals were carried in by the same agent; nay, more, that the hyæna which knawed the bones of an elephant, condescended to pick the flesh from a mouse, and separately break its jaws and legs. This would prove *too much*.

The circumstance of Professor Buckland discovering some rounded pieces or balls, which he considers as the album græcum or fecal matter of the former inmates of the den, at first sight, strengthens his conjecture. Mr Young says, that, "having observed some pieces of bones nearly in the same state, I am not without suspicion that the whole may be portions of bone, decomposed in the cavern, and reduced to their present form by a mixture of water and other ingredients." Without venturing to give an opinion respecting this disputed matter, I may add, that, even viewing it as the fecal matter of hyænas, it too could be carried in by a flood as easily as the os calcis of a water-rat, the jaw of a mouse, the ulna of a lark, or the shoulder blade of a small duck. The evidence proving the Kirkdale cave to have been an antediluvian den, thus seems, in all its parts, so deficient in precision, as to warrant the rejection of that hypothesis it has been produced to support.

In several caves (some in such circumstances occur in the neighbourhood of Kirkdale) the mud does not contain any organic remains. In such cases, the flood must have been truly local, or passed through caverns destitute of the skeletons of wild beasts.

Though the mud in some caves is continuous, in other cases it is distinctly stratified, intimating its introduction to the cave at different intervals. "In one large vault at Oreston, where the quantity of diluvium is very great, it is stratified, or rather sorted and divided into laminæ of sand, earth, and clay, varying in fineness, but all referrible to the diluvial washings of the adjacent country. It is also partially interspersed with small fragments of clay-slate and quartz."—(*Rel. Dil.* 70.)

The last circumstance which I shall notice connected with the mud in caves, is the absence of similarity of colour and composition in different districts. In the mud of the geological deluge, produced from the wreck of Norway and England, or rather of the whole surface of the earth, we might expect the exhibition of a common character in all caves. But when different caves have mud of a particular local character, the inference is obvious, that the causes concerned in its production have likewise been *local*.

IV. *Extinct Animals.*—If ever a sudden, universal, and impetuous flood, sweeped our island in its fury, land animals must have been drowned and carried off, or, as Professor Buckland expresses himself, "every thing that lay without, on the antediluvian surface, must have been swept far away, and scattered by the violence of the diluvian waters."—(*Rel. Dil.* 89.) If we admit the truth of this statement, we should not expect in our country a single skeleton of a native animal, in our gravel, in loam, or in caves. Yet it is admitted that numerous relics of land animals, which lived and died in the country, are generally distributed in gravel, loam and caves. I am inclined at once to conclude, from these premises, that no such geological deluge ever occurred. Nor is other evidence wanting to justify the same conclusion. If these remains "were drifted from other countries to those in which we find them," we may ask, from what countries? Not from tropical regions, for the *species* of hyæna, elephant, and rhinoceros, the remains of which occur in our superficial strata, never were tropical animals, although from *name* the general reader may be betrayed to such an opinion. If these remains "floated backwards and forwards by the flux and reflux of the mighty currents then in motion, before the carcases became putrid, and the bones fell piecemeal into the gravel, as the agitation subsided," then should we expect to find the relics of the animals of arctic, temperate and tropical regions, mingled in the same gravel; in other words, all the laws which regulate the physical distribution of animals would have been violated, and our gravel beds would have been full of the monuments of the rebellion. Yet there is no such confusion; consequently there have been no such mighty currents.

Perhaps the most interesting fact in the history of the relics in our modern strata, is the occurrence in the same gravel of the bones of animals which have become extinct, with such as have been extirpated by the chase, and with such as still inhabit the country. This fact, while it throws great light on the early state of the animal kingdom, may be regarded as the death-blow of the diluvian hypothesis. The extinct animals were, according to Baron Cuvier and Professor Buckland, antediluvian, and perished from off the earth, by the destructive agency of the diluvian waters. The objection to this explanation is unanswerable. The diluvian waters must have drowned *all land animals*; yet many which lived in the reputed antediluvian world, still live and flourish, in the same countries where the remains of their progenitors lie interred. I can find no attempt to explain these facts, except that, in the *Reliquiæ Diluvianæ*, (p. 41.), there is mention made of certain species having "*reestablished* themselves in the northern portions of the world since the deluge;" and by the same author (*Edin. Phil. Journ.* No. xxiv. 308.), of others "that have *repeopled* this country since the formation of the diluvium." The history of this reestablishing or repeopling not being given, we cannot examine the value of the evidence adduced in its support. But we may ask, if the geological deluge ever took place, from whence did the modern animals proceed which *repeopled* the country? If there was any place within the limit of the geographical distribution of our present animals which the diluvian waters did not reach, then it may be supposed, that, independent of the *sudden* and transient nature of the inundation, a place of refuge might have been found, to which these animals retired during the fury of the agitated waters, and from whence they might issue forth to repeople the disolated regions. But, the history of the geological delüge does not warrant such a supposition; nor, even if it did, would the difficulty be removed. We could not avoid drawing the inference, that the place of refuge for the deer and the ox during the catastrophe, might have yielded protection to the gigantic elk and the mammoth. If any great inundation occasioned the extinction of these reputed antediluvian quadrupeds, its ravages must have extended to the other species having the same distribution, feeding in the same meadow, or browsing in the same forest.

Perhaps the abettors of the diluvian hypothesis may have recourse to the Ark as the place where the modern species found a temporary asylum. Still we have to ask the proof of the establishment of that *law of exclusion*, under the operation of which the mammoth and his unfortunate companions suffered extinction? If these were not excluded, we have still to ask, what has become of the *postdiluvian pairs and their families*, of these now extinct species, since they outlived the deluge, but have since disappeared?

Under the conviction that the diluvian hypothesis did not explain the extinction of our early quadrupeds, and that the subject, even in the hands of Baron Cuvier, had not received the elucidation of which it was susceptible, I endeavoured, in my "*Philosophy of Zoology*," to establish the laws which regulate the Physical Distribution of Animals, as a preparation for studying the "Revolutions" which had taken place in the animal kingdom.

I there intimated in general terms (for I could not spare room for more), the effects which the persecutions of man must have produced on the distribution of many species. At the request of my valued friend Professor Jameson, I extended these observations, in the paper on the "*Distribution of British Animals*," which appeared in his Journal.* Subsequent reflection on the subject has only served to confirm the views I have brought forward, and to convince me that we must refer the extinction of these early quadrupeds to the destructive influence of the chase.

It is admitted on all hands, that the relic of the extinct quadrupeds, of those which we know to have been extirpated by man, and of those which still dwell in the country, are coextensively distributed, and must all have lived at the same time in this and analogous countries. From these premises, I may safely draw the following conclusions:—1. That the cause of extinction was not a general physical one, as it did not extend successfully to the subsequently extirpated and recent species. 2. That the cause of extirpation has not extended successfully as yet to the existing species. From the evidence of our observation, and the testimony of history, confirmed by geological documents, I am warranted likewise in the following conclusions:—1. Man is at present carrying on extirpating operations against many species; nor is there room to doubt, that in any age he ever was otherwise occupied. 2. Different species vary in the extent of their resources to resist these extirpating efforts. 3. The *individuals* of many species have been greatly reduced in numbers by these efforts. 4. *All the individuals* of several species have been destroyed by these efforts, in this country, even within the last six or eight centuries. 5. If extirpation has taken place to such an extent, within the period of a few centuries, how manifold must have been its effects during the six thousand years that man has lorded over the creation. To such efforts do I ascribe the extinction of our ancient quadrupeds; and the inductive reasoning which led me to the opinion, carries along with it all the authority of demonstration.

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* Bost. Journ. vol. II. p. 315.

To the explanation which has thus been proposed to account for the extinction of certain quadrupeds, several objections have been offered by Professor Buckland in his "Reply." (No xxiv. 312.) They seem, however, to have originated in imperfect notions respecting the "Distribution of Animals;" and, therefore, readily admit of an answer.

"1. Is it not incumbent on him first to show at what period such animals as these, much too formidable to be overlooked, were ever known to have existed?" I do not think the proof called for with propriety. The events referred to were not sufficiently striking to arrest the attention of the public; and there were no "Journals" in those days.

"2. Can he give any reason why hyænas should have been extirpated at a more early period than wolves, had they ever existed in postdilvian Britain?" Yes. Their resources against the efforts of the sportsman must have been fewer and less efficacious. The proof rests on analogy. The wolf has been extirpated, but the fox remains. The bear has been extirpated, while the badger remains. If we pass from Britain to the Continent, similar proofs occur. The gigantic elk has been annihilated, while the Scandinavian elk remains. If we pass from Europe to America, still there are proofs: the musk ox has perished in Europe, yet it exists in America.

"3. Is it probable that the savage hordes which inhabited Germany before its occupation by the Romans, should have utterly destroyed such powerful animals as the elephant and rhinoceros, as well as the hyæna, from the impenetrable fastnesses of the great Harcynian forest, when animals of the same kind have not yet ceased to abound in the woods of India, and the wilds of Africa, in spite of a farther persecution of nearly two thousand years?" Quite probable. The objection is specious, not solid. Savages are good huntsmen; and those which inhabited the west of Europe were not destitute of energy, as the Romans found to their cost. Those of temperate and cold climates, must follow the chase eagerly, Ceres to them being niggardly. They, too, can commit their depredations with greater effect, aided by the seasons, and the migrations consequent on the changes thereof. But independent of these explanations, I too may ask, How have the wolf and the bear, and the beaver been extirpated from Britain, while, in the nighbouring continent, "after a farther persecution," they still maintain their ground. The same explanation must apply to both cases,—the different facilities of the sportsman to gain his object.

"4. Surely the theory of their extinction by the savage natives, preceding the Roman invasion of these countries, is a matter of the highest improbability; their existence at that time, and subsequent extirpation, is, in the utter silence of Cæsar and Tacitus, and all later historians, and even of tradition, a moral impossibility." I deny that the natives were savages at the period of the Roman invasion; and let the appeal be made to the writings of Cæsar and Tacitus. The silence of the Roman historians as to the destruction of native animals is of little moment. The process of extirpation is gradual, and had commenced long before Romulus and Remus had a being, or the wolf that suckled them. The historians were otherwise occupied; Cæsar, in recording his own achievements, and Tacitus in lauding the deeds of Agricola, and fabricating speeches for Galgacus. As for tradition, the learned Professor rejects the testimony of the Niebelungen, a poem of the thirteenth century, which seems to refer to these extinct animals, because it records, at the same time, some *superstitious notions* of the æra in which it was written. What will become of poor Samuel Johnson's Tour a few centuries hence, with its *second sight?*

There is not in the whole range of this question, a single fact, in the history of animals, yet produced, which justifies, or renders probable the diluvian hypothesis. The whole science of zoology is opposed to it. Nor is phytology friendly to the cause.

If ever a mighty torrent of fresh or salt water committed those ravages on rocks and valleys, which it is represented to have done, the *soil* and *land-plants* must have been the first victims of its fury; and in our gravel, lakes and peat-bogs, we should now find the woods of tropical forests commingled with those which temperate regions produced, as they "floated backwards and forwards by the flux and reflux of the mighty currents then in motion," until they rested in the hollows of the surface, upon the retiring of the waters. The existence of land-plants, at present, on the surface, and the absence of the wreck referred to, attest the non-existence of this supposed catastrophe. Perhaps the plants have "re-established" themselves, and "repeopled" the desolated regions? Where was the spot in which they enjoyed exemption from the fury of the diluvian waters? It must have been within the limits of their geographical distribution; and as each district must have had a separate sanctuary corresponding to the distribution of the *species*, the mighty torrent must have met with many checks in its progress. It may be added, that the animals when they returned to repeople the valleys of denudation, must have been scantily supplied with herbage; and centuries must have elapsed before the washed, waterworn rocks could furnish a support to the vegetable tribes.

Perhaps the advocates of the diluvian hypothesis, in the absence of all support from physical science, may give it as their opinion, that the Deity, immediately after the catastrophe, created new soil, re-created the plants, and re-created *a part* of the species of animals which had been destroyed. Is not the silence of Moses fatal to the conjecture? Would he have failed to record in the sacred volume this second magnificent display of creative power? Perhaps, in this case, there is much need to be reminded of the caution of the poet:—"Nec Deus intersit nisi dignus vindice nodus."

From the preceding statements, I feel myself warranted to conclude, **That the occurrence of the *geological deluge*, in its effects, such as the advocates of the diluvian hypothesis describe, is, like similar well meant inventions of their predecessors, Burnet, Woodward, and Whiston, disproved by the truths of Geology, the truths of Zoology, the truths of Phytology; and contradicted by the authority of Revelation.**

Flisk, 24*th December* 1825.

Art. LII.—*Observations on the Volcanic Formations on the Left Bank of the Rhine.* By G. Poulett Scrope, Esq.—[*Brewster's Journ.*]

The volcanic products which occur in the Prussian provinces on the left bank of the Rhine, are scattered over a district of no great extent, which may be described as bounded on the south and east by the Moselle and Rhine, on the north and west by a line passing from Bonn through Gemund, Prüm, and Bitburg, to Berncastel on the Moselle.

With the exception of the volcanic rocks, the surface of this district is chiefly composed of transition slate, a part of the great Rhine schist formation. In a few places this is partially covered by some of the floetz strata, both sandstones and limestones, to which I can scarcely venture to give a name. The volcanic eruptions have forced their way with apparent indifference, both on the points where the slate is covered by these strata, and where it is exposed. The volcanic energy has not confined itself to this district; analogous formations (though appearing in general to belong to an earlier epoch,) occurring, as is well known, eastward of the Rhine, in the Siebengebirge, the high Westerwald, the Vogelsgebirge, the Rhonebirge, the Meisner, and the Habichtswald, which form altogether a remarkable volcanized band, stretching from west to east in a line parallel to the primitive axis of the Alps, and removed about four degrees to the northward of it. The volcanic country which I have at present to describe has been generally separated by writers into two districts, from the volcanic products being more thickly grouped together at its western and eastern extremities. These divisions are, 1. The group of Andernach, Mayen, and the Upper Eiffel. 2. That of the Lower Eiffel. As I visited them during two different excursions, the first from Andernach, and the second from Spa, I may as well retain this division in their description.

1. *District of Andernach, Mayen, and the Upper Eiffel.*

Upon reaching the summit of the steep and richly cultivated slope which, near Andernach, forms the left bank of the Rhine, you suddenly find yourself in a rude and barren country, presenting a strong contrast to the soft and luxuriant scenery you have left behind, and consisting of an elevated mountain plateau of greywacké slate, across which the deep valley of the Rhine appears but as a narrow trough-shaped channel which the eye overlooks entirely, the plateau being continued at the same level immediately on the eastern side of that river. On the westward the general level rises gradually to the rugged heights of the Upper Eiffel, and it is also partially broken by the narrow and sinuous gorges through which a few tributary streamlets find their way into the Rhine, and still more so by a number of isolated hills of volcanic formation, mostly of a sub-conical form, with which the surface of the plateau is irregularly studded. Some of these hills are very complete volcanic cones, with or without a central funnel or crater, as the Hirschenberg, near Burg-bruhl, the Bousenberg between that village and Olburg, the Poter, Pellenberg, and lastly, the Camillen-berg, perhaps the highest and largest of these hills, which appears to rise above 1000 feet above the level of the surrounding slate plateau. Others are less regular, seeming to owe their want of symmetry to their being thrown up on an uneven surface, as the steep side of a valley, &c. Others form elongated ridges, composed of the mingled products of three or four neighbouring volcanic orifices. Such are the hills above Nieder-nich.

Many have regularly funnel-shaped craters; others are breached on one side by the subsequent emission of a lava stream, and some are still more irregular, and appear to have suffered more or less destruction from the mechanical action of some denudating force since their production. All these cones of every kind are composed wholly of loose conglomerate, or lapillo, containing numerous pumice stones, fragments of a phonolitic lava of clay slate, partly calcined, &c.

Thin beds of these fragmentary matters also occasionally cover the flat parts of the slate plateau in the vicinity of the cones, or occupy a few bosoming hollows in the slopes of its valleys.

Many of these valleys are also filled to a considerable height, often to more than half their total depth, with indurated tufa, called in the dialect of the country *Dukstein* or Trass, of which an immense quantity is quarried on numerous points, and carried down the Rhine into Holland, where it is in great request for buildings. The lower part of the mass is universally the most solid and compact, and hence is preferred by the quarrymen. It passes gradually into loose arenaceous tufa towards the upper part of the deposit. This tufa resembles extremely that of Naples (particularly of Capo di Monte and Posilipo). When freshly quarried, it is thoroughly saturated with water, which is driven out by every blow of a hammer upon it. In this state it is of a dull bluish black colour, but, on drying, it assumes a shade of light grey. It appears to be almost wholly composed of fragmentary pumice, and is evidently a conglomerate. It contains also fragments of a slaty or phonolitic, and of amorphous basalt, of burnt clay-slate, and a great quantity of carbonized wood, not in fragments or beds, but consisting of whole trunks or branches, which penetrate the rock in all directions. The condition of this wood is very nearly that of common charcoal, but it pulverizes more readily, and often of its own accord, on exposure. In the valley of Burg-bruhl the trass rests sometimes immediately on the clay-slate, but, on other points, a bed of calc-tuff intervenes, the deposit of some mineral spring prior to the deposition of the tufa. A similar incrustation occasionally overlies the trass, and has enveloped fragments of pumice, forming a species of calcareous tufa. The indurated tufa is sometimes divided into massive beds by intervening layers of loose pumice or lapillo, and fragmentary clay-slate.

On ascending the valley of the Bruhl, I found this trass deposit occupying it to a great depth the whole way from its embouchure in the valley of the Rhine, up to the foot of the Feitsburg, one of the hills which form the circumference of the lake of Laach; from whence this, as well as many other streams (if they may be called so) of tufa are derived.

The basin of the lake of Laach is nearly circular and crateriform, encircled by a ridge of gently sloping hills of no great elevation. They are composed of irregular beds of loose tufa, containing numerous fragments, and some very large blocks of a variety of lava-rocks. Those which are most abundant are of a basalt with very large and regular crystals of black augite, and of olivine. Fragments also occur of trachyte, sometimes of a whitish yellow colour and conchoidal fracture; at others, of a coarse grain, consisting solely of crystals of glassy felspar and hornblende. Some fragments are also found similar to those which are common in the conglomerates of Somma, composed of an agglomeration of crystals of mica, nepheline, meionite, Vesuvian, and other rare minerals. No lava-rock appears in place within the interior of the basin, and on its exterior, the only rock of this nature which shows itself on the surface in the form of a regular current of lava, is that in which the millstone quarries of Nieder-mennig are worked. This stream certainly flowed from the crater of Laach, the ridge of which suffers a depression on that side. The eruption which produced it was probably the last, not only of this particular vent, but perhaps of the whole district, as its surface has an air of great freshness, and is not yet entirely clothed with vegetation.* The rock of which it consists is basaltic, with very few visible crystals of augite, and extremely cellular, the cavities being very small and irregular. It is divided into rude columns at the lower part of the current, which is much more compact than the upper, but still cellular. It is here so hard as to be in great request for millstones, which are exported to Holland in great numbers, and from thence find their way to England. It envelopes numerous fragments of quartz (always more or less vitrified and cracked,) of granite, and other problematical rocks like those described above, as occuring in the conglomerate, crystals of lazulite, &c.

The volcano of Laach appears, like so many in other countries, to have produced at first trachytic, and afterwards basaltic lavas. The trass and the pumice conglomerates, which chiefly compose its surrounding eminences, belong decidedly to the former class of products; and, though trachyte, as a rock, does not, I believe, show itself *in situ*, it probably exists, concealed by the fragmentary strata of the hills, or the thick woods which cover their slopes, and render it difficult to examine their composition. The origin of the trass has been variously accounted for, and sometimes ascribed to deluges and other similar hypothetical events. It appears to me to derive simply from an ordinary modification of the volcanic phenomena. The pulverulent matter, of which it was principally composed, mixes into a retentive paste or clay with water, so indeed, as to be used for making pottery, where it is found in a loose state. In this state it was ejected by the volcano, and thrown up as usual into a circular or elliptical ridge around the orifice. The rain, which falls generally in great abundance after the termination of an eruption, mixed with these trachytic ashes, must often have formed an impermeable crust at the bottom, and upon the sides of this cavity. Hence the water that drains down these slopes would accumulate into a lake continually increasing in depth, until either the pressure of its waters breaks down the banks on some one side, or a fresh eruption from below displaces it. In either case, a breach being made in the circumference of the crater, the contents of the lake must rush out in a violent *debacle*, carrying off great quantities of the fragmentary matter of the hills through which the water bursts, and filling with these alluvial deposits the vallies by which it escapes on the plains at the foot of the volcano.

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* This may have been the eruption recorded by Tacitus (xiii. lib. Annal.) as having ravaged the country of the Jutiones, near Cologne, in the reign of Nero.

This process may be many times repeated from the same volcanic orifice, and, I think, is without doubt the real history of the tufas of the left bank of the Rhine, as well as of those of the Mount-Dor, Cantal, and of some parts of Italy. Whether the mass hardened afterwards or remained incoherent, appears to have depended chiefly on the quality of the ashes, and their intimate commixture with the water. This induration is evidently a chemical process, analogous to the *setting* of cements and mortars. The mud eruptions (*tepetate*) of Quito, and the tufas of Iceland, are produced by the same train of circumstances in the present day. As the filling up of the crater must usually be a slow process, a sufficient interval will often occur before the lake bursts through its sides, either by its own weight, or the occurrence of an eruption, for the slopes to be covered by vegetation, and even by whole forests of trees, which, when the banks give way, will be hurried along, and buried within the torrent of mud, (or liquid tufa,) where they are afterwards carbonized, probably by long exposure to the moisture which penetrates the whole rock.*

With regard to the trass of Laach and its vicinity, this explanation is peculiarly applicable; and the lake would, even at this day, be subject to rise until it burst its bank, but for an artificial channel, or emissary, cut for its drainage by the monks of the abbey of Laach, a picturesque ruin which stands on its western side. Currents of tufa appear to have been discharged in this manner from many points of the circumference of the lake. Those that issued on the eastern side occupied the valleys of the Brohl, and other streams which empty themselves into the Rhine; the remainder inundated the slate plateau in the direction of Niedermennig, Bell, Olburgh, and Craft, and covered it more or less with beds of compact tufa, which alternate with others of similar composi-

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*Since we hear that numerous trees are found *in a carbonised state*, amongst the substances blown into the air by some of the paroxysmal explosions of the Javanese and Polynesian volcanoes, it remains doubtful whether this character is always owing, as in this case, to torrefaction by volcanic heat, or occasionally to long maceration in water. Are these trees only charred on their surfaces, or *throughout*, like the surturbrand?

tion, but loose and incoherent, probably deriving from the fragmentary ejections of the neighbouring vents.

A cavern within the basin of the lake of Laach gives out a considerable volume of carbonic acid gas, presenting all the phenomena of the Grotta del Cane. There are also many mineral springs in the vicinity, as at Tonigstein, and near the Brohl, strongly impregnated with the same gas, which is usually the latest product of an otherwise extinct volcano.

At some distance from Laach, towards the south-west, and between the villages of Bell and Mayen, rises another group of cones, containing two or three irregular crateriform basins, from which different mud streams appear to have flowed, covering the slate plateau in their neighbourhood with their deposits. These volcanic vents differ, however, from that of Laach, in having produced leucitic lavas, and, consequently, their conglomerates are of a different character, resembling exactly the peperino of Monte Albano. Such is the rock quarried near Bell, and called *bak-ofen-stein.* It is in request for lining ovens, from its capacity of resistance to fire, which it owes to its being almost wholly composed of lucite in a fragmentary state. It encloses many small white farinaceous leucites, fragments and blocks of leucitic lava, of burnt clay-slate, and large broken plates of mica.

The leucitic phonolite spoken of by Keferstein, as existing in massive beds near Reiden and Meyr, I presume to derive from this system of vents.

Further to the south, and near the village of Kruft, rise three other smaller cones, covered with vegetation, and with faint traces only of craters. Other cones, and some of a large size, are visible to the westward of Olburg, but my time did not permit me to examine them in detail. On the whole, the volcanic products of Andernach, and the Upper Eiffel, seemed to me to bear the greatest analogy to those of Italy, particularly of the Campagna di Roma. The points on which they differ, are the result of the former volcanoes having broke forth on a high and dry slate plateau, the latter from a submarine alluvial shore. In both these districts, as well as in the Campi Phlegræi, it is remarkable that the same, or at least very neighbouring vents, have produced *trachytic, leucitic*, and *basaltic* lavas.

2. *District of the Lower Eiffel.*

The group of volcanic vents which occupies this district is in immediate contact with that of Laach and the Upper

Eiffel, though the points on which eruptions have taken place are rather more thickly sown towards the western limit, particularly along the course of the river Kyll, than at its eastern extremity. The epoch of their activity appears also to be equally recent, dating at least since the formation of all the vallies of the country, into which their lava-streams have invariably flowed, usurping the beds of the rivulets, which but in very few instances seem to have had force or time enough to execute a new channel to any depth below the level of their former one. Indeed, such is the freshness of aspect which many of the volcanic rocks of this district exhibit, that it requires the silence of all historical records on the subject, to persuade us that they have not been produced within the last 2000 years. Nor is such evidence, indeed, at all conclusive. It is probable that accounts of phenomena of this kind would rarely reach the meridian of Rome from distant and barbarous districts, unless when they were of a most destructive and terrific character, such, perhaps, as that spoken of by Tacitus, and mentioned in a former page; and if any such occurred, during the middle ages, all traditionary account of them may well be supposed to have perished with so much of other and more valuable information.

The volcanic eruptions of the Lower Eiffel have burst through the exposed surface of the transition slate formation on many points, and on others through masses of flœtz strata, which overlie the slate, throughout a considerable part of this district; these latter formations are red sandstone, shell limestone, and quader-sandstone. Some of the vents have emitted currents of augitic lava (basalt); others have confined themselves to the discharge of fragmentary matters. The latter principally, and in some instances almost entirely, consist of broken greywacke, slate, and sandstone, more or less affected by heat, and pulverized. It is probably owing to the clayey nature of these fragments, when reduced to great fineness, that the craters of this country have nearly, without exception, become reservoirs of water, or *Maare*, as they are called by the natives. Most of them still have small lakes or peat-marshes at their bottom. Some have been drained for the sake of cultivation; a few appear to have undergone the same process by natural means, either from the lake rising till its weight burst through the banks encircling the crater, or from the slow erosion of the stream by which it discharged itself. In the last case, the sides of the basin are cut through by this natural emissary, as is seen in the Meerfelder and the Drieser maare, as well as in those near Strohn and Walsdorf. In the other case, the regularity of the basin has been more or less destroyed by the bursting of its banks, and considerable deposits of trass, or rather of peperino, have been formed, evidently aggregated by means of water. Examples of this are met with in the remains of craters near Stiffler, Schalkenmeyrener, and Rockeskill. On those points where lava has been emitted in a liquid form, a regular crater is rarely to be seen; at least at the source of the lava-current. There exists, however, always one or more such craters in the vicinity of this source, which appear to have produced violent aeriform explosions, and ejected scoriæ and ashes, while the lava was flowing from the neighbouring orifice. The force and rapidity of these explosive discharges of confined vapours, is attested by the great size of the cavities they have hollowed out of the solid greywacké strata. That of Meerfeld, for instance, one of the largest, measures above 500 feet from the surface of the lake (which is itself 150 feet in depth,) to the average height of the ridge which encircles it, and its diameter can fall very little short of a mile. The quantity of fragmentary ejections heaped round these basins is not at all proportionate to their extent. The greater part consists of slate and sandstone, in pieces of every size, and appearing half-burnt, probably from having fallen repeatedly upon the surface of lava within the vent whence the explosions of vapour were discharged.

The accompanying sketch of a map of part of this district will convey an idea of the relative disposition, and peculiar characters of these sites of volcanic activity. The most westerly point on which any traces of volcanic eruption are met with, is Ormont, where, upon the wild and elevated transition plateau of alternating slate and quartz-rock, two small cones are seen to rest. They are in contact at their bases, and have neither craters nor visible lava-currents. The scoriæ and fragments of which they are composed are basaltic, with much augite and large plates of brown mica. Isolated crystals and pieces of augite also occur, some nearly as large as the fist.

At no great distance to the east of Ormont, the transition rocks are concealed by strata of red sandstone, inclined at a high angle, with an easterly dip. At the village of Steffler, these are in turn covered by other sandstone strata, which appear to belong to the quader-sandstone: resting upon these, to the south of the village, rises a volcanic cone, composed of scoriæ and puzzolana, partly incoherent, partly compacted into a peperino. Steffler is built on strata of this latter kind, which, however, by their inclination, are proved to have been deposited by an alluvial torrent descending from another hill N. E. of the village, which still exhibits a large circular crater on its summit.

To the S. E. of Steffler, lies a small *maar*, or crater-lake, which has been once drained, and since filled again by a dam thrown across the channel of discharge, on which a mill is situated.

The village of Roth is built on a current of basalt deriving from the cone which rises above it, and which has also emitted a considerable mass of lava towards the north and west. A small cavern, the mouth of a deep fissure in one of these lava-currents, half-way up the side of the cone, is noted for exhibiting a phenomenon, which I have met with elsewhere, in many instances, amongst volcanic formations. The floor of this grotto was paved with a thick crust of ice, when I visited it at noon on a very hot day at the latter end of August. During the summer, the peasants of the neighbourhood say it is always found there, while in the winter there is none; but, on the contrary, that the shepherds creep into the cavern for warmth. The following appears to me the most plausible mode of accounting for this curious fact: The cave is probably the mouth of one of those arched galleries which are so frequently met with under currents of lava in Iceland, Bourbon, and elsewhere. If the other extremity of the gallery communicates with the open air at a much lower level, for instance at the foot of the cone, or where the lava stream terminates in the plain below, a current of air must be continually driven through this passage from the lower to the upper extremity. In its passage, it would be thoroughly *dried* from the absorbent nature of the rock (which is perhaps partly owing to the sulphuric or muriatic acids it contains), and the evaporating effect of this current on the wet floor of the grotto from which it issues, which is moistened by some superficial rill, will be sufficient to coat it with ice in summer, since the more rarified by heat the external air, the more rapid will be the current of cool dry air, and, consequently, the evaporation. In winter, a similar draught of air, though less rapid, will be produced; and taking the temperature of the rocks through which it passes, which, from the depth of the gallery, will be about the mean annual temperature of that climate, must appear warm compared with the external air, to the shepherds who seek a shelter at the mouth of the fissure.

The cone of Roth connects itself with a smaller ridgy hill prolonged towards the Kyll, which has given rise to three or four small distinct streams of basaltic lava.

On approaching the Kyll towards Gerolstein, the traveller is struck by the appearance of an elevated plateau formed of Jura limestone in horizontal strata, resting on the quader-sandstone, and bounded by a range of picturesque and craggy cliffs, with a talus of massive debris at their base. From the surface of the plateau rise four large volcanic cones, besides smaller eminences of a similar nature. One has given rise to a current of basalt, which descends the steep cliffs of limestone in a sort of cascade, on the western side, occupies a small bottom, and, winding round the base of the range of rocks, reaches the channel of the Kyll at Sarsdorf.

The two largest cones of this plateau, lie N. W. of Casselburg, a romantic ruin of great picturesque beauty about two miles N. of Gerolstein.

Round Rockeskill, there are traces of another aqueous formation of peperino similar to that of Steffler, and appearing to have originated in the hill immediately behind that village. Further north, the Waldsdorfer Kopf is a very regular cone, and at its foot lies a crater-basin, once a lake, but now reduced to a peat-moss. The cone has emitted one of the largest currents of lava of this district. It has flowed towards the west, and reaches nearly to Hillesheim.

Arnsberg is a large and complete cone, which has also produced much lava. Eastward of Waldsdorf lies the Dreiser Maar, a wide crater, which has been artificially drained. Masses of olivin, often of three or four pounds weight, and as large as a man's head, are found in the fragmentary strata which form the sides of this basin. Part of this encircling ridge rises into a high cone on the south-west, and this is again connected with a third hill above Dockweiler, which exhibits a well-characterized crater at its summit, and has sent forth powerful streams of basaltic lava. The road from hence to Daun, leaves on the right three or four considerable cones near Nerod and Steinborn. They consist in a great part of lava which has burst from their summits or flanks, and flooded the lowest levels of the surrounding plain.

On the east of Daun, a massive and elevated bed of basalt, bordered by abrupt cliff-sections, in which a rudely columnar configuration is visible, descends towards the town from a higher eminence at its eastern extremity, which is composed of scoriæ, and exhibits vestiges of a crater. This bears the appearance of being the least recent of all the volcanic formations of the neighbourhood.

South of Daun rises a group of hills which appear, as they are mounted, to be solely composed of greywacke slate, and in which, consequently, no volcanic appearance could be anticipated, when, on reaching the summit, the traveller suddenly finds himself on the edge of a deep circular lake-basin, evidently drilled through the greywacke by repeated and powerful discharges of subterranean vapour. There are three of these maar strung together on a line, in a N. S. direction, and in immediate contact, the same ridge forming the barrier of two neighbouring craters. The fragments of which the surrounding slopes are formed, consist chiefly of slate partially calcined, the remainder of augitic scoriæ. A large rock of greywacke slate, evidently *in situ*, projects from the bottom of one of these basins. The water in the three lakes appears to stand at the same level, and they probably communicate by means of some fissures in the intervening rocks. One only, the Schalkenonchrener maar, has any visible outlet, and there are traces of trass-streams in that direction.

A few miles farther to the south, the Polvermaar of Gillenfeld, is met with: a magnificent oval basin, presenting exactly the same general characters as those just described, but remarkable for its large dimensions and extreme regularity. The ridge of fragmentary matters, which girds it in, is without a break, and nearly every where preserves a uniform level at about 150 feet above the water surface. The depth of the lake is above 300 feet; the sides slope in the interior at an angle of about 45°, on the exterior of 35°. Immediately at the foot of the cone of the Polvermaar, on the south side, rises a hill containing a much smaller crater, with a peat-bog at its bottom.

Still farther south, between the villages of Strohn and Trittschied, is a double cone of large dimensions. It has two considerable craters, both broken down towards the N. W. The southernmost is large and circular, and bottomed by a morass. The other has produced a current of basaltic lava, which, after forming some considerable hummocks in a N. W. direction, turns its course along the bed of the neighbouring rivulet to the S. W. and occupies its channel to a distance of two miles or more, crossing the great Coblentz road.

But unquestionably the group of volcanic vents, which presents the greatest interest of all in the Eiffel district, is the Moseberg near Bettenfeld, with the neighbouring Meerfelder Maar. The Moseberg is one of the highest hills of the whole country. Its base up to a considerable elevation above the level of the plain around, consists of greywacke slate and red sandstone. Its summit is formed by a triple volcanic cone, the accumulated ejections of three small craters, which remain very distinct. The two most northerly ones are entire, and reduced to the state of peat-marshes. The third has been broken down on its south-east side by a current of lava, of very recent aspect, which, issuing from the breach, descends the slope of the mountain in a stony flood, until it reaches the bed of a small river below.

The lava and scoriæ of these cones, have enveloped a great quantity of half-fused fragments of sandstone and slate. The circular crater, called the Meerfelder Maar, is remarkable for its vast size and depth. It has been hollowed out of both the transition-slate and red sandstone, forming the north base of Moseberg ; and the steep walls which encircle it, exhibit, on many points, the abrupt sections of these rocks, which are only partially covered by a sprinkling of ashes, puzzolana, pulverized slate, and other fragmentary matter. The bottom of this cavity is occupied by water to about a third of its superficial extent ; the remainder is a plain, on which the village of Meerfeld is seated.

The most southerly point of this district, on which volcanic products have been met with, is the vicinity of the baths of Bertrich, a village seated at the bottom of the deep and narrow mountain gorge of the river Isbach, which flows at the distance of a few miles into the Moselle.

Here a lava, which has congealed into an exceedingly hard, tough, and compact basalt, full of crystals of olivin and augite, appears to have been emitted from clefts in the greywacke, on three or four neighbouring points, upon the very brink of the steep slope, or rather precipice, which forms the northern flank of the valley. Very few aeriform explosions seem to have taken place, since scarcely any scoriæ were ejected, and the few that occur lie in beds *upon* the surface of the lava, around its three principal sources, and were therefore thrown up *after* its emission. At each of these points is a very small cone. The most easterly, called the Fackerkohl, has an evident crater encircled by rocks of basalt covered by scoriæ. From hence a stream of basalt may be traced uninterruptedly into the bottom of the valley, (which is here about 600 feet in depth,) falling in a sort of indurated cascade over the almost perpendicular cliffs of transition slate.

The next cone, called Falkenlay, consists of a mass of basalt covered by a deep bed of scoriæ, and also gives rise to a copious current of basalt, which descends into, and has usurped the channel of the Isbach to some distance, both up and down the stream. The third point of eruption presents two very low and small cones, formed entirely of scoriform basalt, and appears to have produced a current of no great magnitude, which may be traced at least part of the way down the nearest ravine into the main valley below.

The exceeding crispness of the scoriæ of this locality, particularly of the Falkenlay, is remarkable. Fragments of greywacke, greywacke slate, and quartz, partly fused, and graduating on these parts into the basalt, are enclosed in great abundance by this scoriform lava rock.

At the bottom of the valley it becomes evident that the mountain torrent called the Isbach has cut through and carried off the greater part of the basalt streams which once filled its channel to a considerable height, throughout an extent of more than a mile above, and rather less than this below the village of Bertrich. Patches only of basalt are left now on either side of the present bed of the river, and most usually in the concave elbows of the valley, but of these some present cliffs fifty feet in height. The lower part of these masses of basalt is regularly columnar, the columns being divided by frequent joints, from two feet to six inches apart. Where they have been long exposed to erosion from the torrent, the angles of these short prisms yielding sooner than the nucleus, the columns appear formed of rude and flattened spheroids piled upon one another. This is, in short, an example of the columnar divisionary structure passing into the globular, by the increase of the number of joints. An arched passage, which goes by the borrowed name of Fingal's Cave, nearly a mile above Bertrich, exhibits this structure in the most perfect manner. It has evidently once formed the channel of the little torrent which now runs on one side of it, and which has thus partly worn away the columns, till they are reduced to mere piles of balls.

The eruptions of these three or four contiguous vents were probably simultaneous, or very nearly so. The lava streams produced by them can be, with difficulty, distinguished from each other, all uniting in the valley below, and the basalt of all is identical in mineral character. It seems probable that the thermal springs of Bertrich-bad owe their warmth to having percolated through some mass of lava not yet quite cooled in the interior of the schist rocks, occupying perhaps the prolongation of the fissures through which the lava streams were expelled. It may be presumed, indeed, that the temperature of these springs is diminishing in consequence of the gradual cooling of this mass. It is at present below blood heat, but appears by its ancient celebrity, to have been formerly much higher. Since the year 1773, it has not, I believe, been analyzed. If the taste is to be trusted to, it has now few or no mineral ingredients. The savour is, as nearly as possible, that of pure fountain water.

I cannot quit this spot without mentioning that the beauty of the scenery on the banks of the Moselle, south of Bertrich, and indeed along its whole course through the transition slate formation between Treves and Coblentz, is scarcely to be paralleled by the far more known and vaunted beauties of the Rhine, even on its most picturesque parts. The want of a post-road along its banks, and the numerous windings of its course, which renders its navigation tedious, has alone prevented the charms of the Moselle from sharing the celebrity of its more travelled neighbour. In a geological view this river is not devoid of interest. Its valley is worn across the whole transition slate district in a direction transverse to that of the stratification. The sinuosities which have been occasioned by this circumstance are so extreme, that in some instances, as near Zell, the river returns to within a few hundred yards of a point it left sixteen miles behind, according to the course of its current. Such windings are not uncommon among rivers meandering slowly through flat alluvial plains; but in a rocky mountain district, where the banks rise steeply to a height of 12 or 1500 feet above the river, they are more remarkable. In either case they are wholly incompatible with the notion of a rapid and powerful excavating force, such as a debacle or deluge, and can only be referred to the slow and gradual erosion of the river itself, which is yet continuing to deepen its bed, and to hollow out still further the concave elbows of its valley, by the double action of its *vertical* and *lateral* abrasive force. If the valley of the Moselle is thus incontestably shown to have been excavated by the slow agency of causes similar to those still in operation, why should we look for another and hypothetical agent to account for that of the neighbouring Rhine, the dimensions of which are greater only in proportion to the greater mass of its waters, and the different solidity of the rocks through which it has worn its channel. I need not carry on the argument from the Rhine to other rivers. All this is in fact a digression, and out of place, for which I am bound duly to apologize.

Having now given a brief sketch of the principal volcanic products of the Eiffel, I need not prolong this paper, already, I fear, swelled beyond its proper limits. There occur a few other vents in the vicinity of Ulmen, Kellberg, Adenau, and Boos, which form the connecting links between this district and that of Andernach. Some of these I did not visit, but from those which I saw, as well as from Steinenger's account of the others, they appear to be mere repetitious of the least interesting of the cones and maare already mentioned.

Upon the whole, though the vestiges of volcanic phenomena to be observed in the Prussian provinces on this side of the Rhine, offer, without doubt, a highly interesting field of study to the geologist, yet they cannot be recommended as types of volcanic formations to those who, without visiting other more distant vents of subterranean energy, either active or extinct, might seek, in the short tour between Spa and Coblentz, to acquire a general knowledge of the effects of this class of natural agents. In this view, as in every other, they are far less instructive than the analogous formations of Auvergne, the Velay, and Vivarias, where almost every possible modification of the volcanic phenomena is to be clearly traced, and on a much larger scale. In the Rhine districts, there is a comparative littleness, and an appearance as if the volcanic energy had been damped and impeded by the mass of transition and secondary strata which it had to pierce, and still more so perhaps by the fragile nature of the greywacke slate, which, shattered and pulverized by the first few aeriform explosions of every eruption, would accumulate in prodigious volumes above and within the vent, and speedily stifle its further activity. The same circumstance will account both for the general paucity of lava produced by these volcanoes, and for the numerous deep and wide craters, the formation of which, by the rapid and explosive discharge of subterranean vapour, will, it is evident, have been facilitated in proportion to the fragility and incoherence of the superficial rock.

ART. LIII.—*Instructions for Collecting Geological Specimens.*[*]

IT so often happens that specimens sent from distant places, by persons unpractised in geology, fail to give the instruction which is intended, from the want of attention to a few necessary precautions; that the following directions may perhaps be useful to some of those, into whose hands these pages are likely to fall. It will be sufficient to premise, that two of the principal objects of geological inquiry, are, to determine,—1st, the nature of the *Materials* of which the earth is composed; and, 2dly, the relative *Order* in which these materials are disposed with respect to each other.

1. Specimens of rocks ought not, in general, to be taken from loose pieces, but from large masses in their native place, or which have recently fallen from their natural situation.

2. The specimens should consist of the stone unchanged by exposure to the elements, which sometimes alter the characters to a considerable distance from the surface.—Petrifactions, however, are often best distinguishable in masses somewhat decomposed; and are thus even rendered visible, in many cases, where no trace of any organized body can be discerned in the recent fracture.

3. The specimens ought not to be too small.—A convenient size is about three inches square, and about three-quarters of an inch, or less, in thickness.

4. It seldom happens that large masses, even of the same kind of rock, are uniform throughout any considerable space; so that the general character is collected, by geologists who examine rocks in their native places, from the average of an extensive surface:—a collection ought therefore to furnish specimens of the most characteristic varieties;—and *the most splendid specimens are, in general, not the most instructive.* Where several specimens are taken from the same place, a series of numbers should be added to the note of their locality.

5. One of the most advantageous situations for obtaining specimens, and examining the relations of rocks, is in the

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[*] Extracted from "An Account of some Geological Specimens from the Coasts of Australia. By Wm. Henry Fitton, M. D., F. R. S., V. P. G. S., &c. From the Appendix to the 'Narrative of a Survey of the Inter-tropical and Western Coasts of Australia,' &c., by Captain Philip Parker King, R. N., vol. ii. p. 566, &c."

sections afforded by cliffs on the sea-shore; especially after recent falls of large masses. It commonly happens that the beds thus exposed are more or less inclined; and in this case, if any of them be inaccessible at a particular point, the decline of the strata will frequently enable the collector to supply himself with the specimens he wishes for, within a short distance. Thus, in the subjoined sketch, (Pl. III. Fig. 7,) which may be supposed to represent a cliff of considerable height,—the observer being situated at *a*, the beds *b*, *c*, *d*, though inaccessible at that place, may be examined with ease and security, where they successively come down to the shore, at *b′*, *c′*, and *d′*.

6. To examine the *interior* of an unknown country, more skill and practice are required: the rocks being generally concealed by the soil, accumulations of sand, gravel, &c., and by the vegetation of the surface. But the strata are commonly disclosed in the sides of ravines,—in the beds of rivers and mountain streams; and these, especially where they cross the direction of the strata, may be made, by careful examination, to afford instructive sections.

7. Among the occasional components of the strata, the remains of organized bodies,—shells, corals, and other zoophytes,—the bones and teeth of animals,—fossil wood, and the impressions of vegetable stems, roots, or leaves, &c., are of the greatest importance; affording generally the most marked characters of the beds in which they occur.—These should, therefore, be particularly sought after, and their relative abundance or rarity in different situations noticed. The petrified bodies should, if possible, be kept united with portions of the rock or matrix in which they are found; and where they are numerous,—in sand, clay, or any moist or friable matrix,—it is in general better to retain a large portion of the whole mass, to be examined afterwards, than to attempt their separation at the time of collecting.

8. The *loose materials* which are found above the solid rocks, in the form of gravel, silt, rolled pebbles, &c., should be carefully distinguished from the *solid strata* upon which they rest. And the more ancient of these loose materials, found on the sides or summits of hills, &c. should be distinguished from the recent mud, sand, and gravel, brought down by land-floods, or by rivers. The bones and teeth of animals are not unfrequently found in the more ancient gravel; and the collection of these remains from distant quarters of the globe, is an object of the greatest interest to geology.

9. Besides a note of the locality, there ought, if possible, to accompany every specimen, a short notice of its geological circumstances; as—

Whether it be found in large shapeless masses, or in strata?

If in strata,—what are the thickness, inclination to the horizon, and direction with respect to the compass, of the beds?—[If these cannot be measured, an *estimate* should always be recorded, while the objects are in view.]—Are they uniform in dip and direction?—curved, or contorted?—continuous, or interrupted by fissures or veins?

Is the whole cliff, or mass of strata in sight, of uniform composition?—or does it consist of different kinds of stone?

If the strata be different,—what is the order in which they are placed above each other successively?

10. A *label*, distinctly written, should accompany every specimen, stating its native place, its relative situation, &c., &c. And these labels should be connected with the specimens immediately, on the spot where they are found.*—This injunction may appear to be superfluous; but so much valuable information has been lost to geology from the neglect of it, that every observer of experience will acknowledge its necessity; and it is, perhaps, in practice one of the most difficult to adhere to.

11. A *sketch* of a coast or cliff, however slight, frequently conveys more information respecting the disposition and relations of rocks, than a long memorandum. If numbers, denoting the situation of the specimens collected, be marked upon such sketches, much time may be saved at the moment of collecting. But in all such cases, the memorandum should be looked over soon afterwards, and labels distinctly explaining their situation, &c., be attached to the specimens themselves.

12. The specimens should be so packed, that the surfaces may be defended from exposure to air, moisture, and friction: for which purpose, if strong paper cannot be obtained, dry moss, or straw, or leaves, may be employed. Where paper is used for wrapping the specimens, they are best secured by fastening the envelope with sealing-wax.

Lastly, The collector must not be discouraged, nor be prevented from collecting, by finding that the place which he may chance to visit in a remote situation, has not a striking appearance, or the rocks within his view a very interesting character; since it frequently, and even commonly, happens, that facts and specimens, in themselves of very little importance, become valuable by subsequent comparison; so that scarcely any observation, if recorded with accuracy, will be thrown away.

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* It is useful to mark on the labels the day, and even the hour, when each specimen is collected. This, with a corresponding note in the memorandum-book, will be found to assist the memory, and prevent confusion.

The *Instruments* required by the geological traveller will vary, according to the acquirements and specific objects of the individual. The most essential are:—

The *Hammer;* which, for general purposes may be of the form represented, (Pl. III, Fig. 6). The head should be of steel well tempered, about four inches from the face to the edge, and 1¼ inch square in the middle; the face flat,* and square, or nearly so; the edge placed in the direction of the handle. The orifice for the insertion of the handle, oval, a very little wider on the outer side than within; its diameters, about 1 inch vertically, and 7/10 across; the centre somewhat more than, ½ inch from the face. The handle should be of ash, or other tough wood; not less than 16 inches long; fitting tight into the head at its insertion, without a shoulder; and increasing a little in size towards the end remote from the head, to prevent its slipping.—It should be fixed in the head by means of a thin, barbed iron wedge.

For *trimming* specimens, smaller hammers may be employed:—The form of the head, recommended for this purpose by Dr M'Culloch,† is rectangular. The dimensions of the face may be 1 inch by ¾; the height 2¼. (Pl. III, Fig. 5).

It will be expedient to have always some hammers, (or at least the heads,) of different sizes, in reserve.

A small *miner's pick* is useful for cutting out, and splitting portions of slaty rocks; or for obtaining specimens of clays, &c.

A small *stone-cutter's chisel.*—A chisel with a handle, of the form here represented, will often save the hand of an inexpert collector, and better enable him to direct his blow. (Pl. III, Fig. 4).

For *Packing the Specimens.*—A stock of strong *paper. Sealing-wax. Writing-paper*, cut into *labels.* Thick *gum-water*, to cement the labels to the specimens.

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* A rounded head is most durable. W.

† "*On the forms of Mineralogical Hammers.*" Quart. Jour. vol. xi. 1821, p. 1, &c.

For the *Conveyance of specimens.*—A large *bag of leather*, with straps for the shoulders. Strong canvass *bags*, *of smaller size*, are very convenient for subdivision and arrangement.—For the protection of crystals, or delicate petrifactions, &c. *wool* or *cotton* are necessary; and small *wooden boxes* (like those used for holding wafers) are sometimes required. For distant carriage, strong *wooden boxes*, *casks*, or *baskets*.

The following are either essential, or useful in various degrees, for obtaining and recording observations.

Pocket Memorandum-Books, of sufficient size to admit sketches.

*A Pocket Compass.**

A Measuring-tape, of fifty feet, or more.

A Telescope.

A Camera Lucida.

A Box of Colours.

The best *Maps* should always be sought for:—And, the true economy to the traveller being that which saves time, it is best to mark, or even to colour the map, in the field. Notes inserted on imperfect maps, or deduced afterwards from memoranda, are less authentic; and the process is frequently neglected.

Portable-Barometers, with detached thermometers, are desirable; and the best instruments are ultimately the cheapest. But, unfortunately, barometers of every construction are very easily damaged or deranged.—Minute accuracy, however, in the determination of heights, though very interesting to physical geography, is comparatively of little importance to the geologist.

If the collector be a surveyor, he will know best to what purposes a *Pocket Sextant*, or a *small Theodolite*, is applicable:—the measurement of distances,—of heights,—and of the inclination of strata, &c.

ART. LIV.—*Sketch of the Mineralogy of Gay Head, and of Bird Island, with a Description of a large Sun Fish, caught in Vineyard Sound.* [Extracted from a Letter from General H. A. S. Dearborn, to one of the Editors.]

DEAR SIR,

HAVING recently visited Gay Head, a southwestern promontory of the island of Martha's Vineyard, and one of the most remarkable and interesting geological localities in New England, I send you specimens of the minerals which were collected.

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* A clinometer, tools for drilling, a *copper* priming wire, and gunpowder W.

The front of the cliff has an altitude of about one hundred and fifty feet, with a base of near four hundred yards. The strata of red, blue, yellow, grey, and white clays and ochres, white siliceous sand, mingled with a small portion of pipe clay, ferruginous stones, ores of iron, bituminous wood and coal, have a dip to the northeast of between 40 and 50 degrees.

These strata are from ten to twenty feet thick, each containing, either in distinct masses, or admixtures of several, and sometimes of all, the various substances, above named; and are covered from five to twenty feet, with sand, gravel, rolled pieces of quartz and boulders of granite and sienite, from small pebbles, to masses of hundreds of tons' weight.

The boulders, of the larger size, generally appear above the surface of the ground, but many of them are entirely buried in the gravel and sand.

The base of the cliff being constantly abraded by the waves of the ocean, which roll in upon the beach, with tremendous violence, during heavy gales of wind, the superincumbent masses of clay and stones are constantly tumbling down, and the former gradually liquifying, is swept away by the receding tide, and leaves the shore covered with gravel and large stones, which formerly rested upon the surface of the peninsula; while the transverse section of the variegated strata of clay, sand, and bituminous wood and coal, are perfectly exposed on nearly a perpendicular precipice, and have given to this remarkabe head-land its very appropriate cognomen.

At the eastern base of Mount Pleasant,—the highest land in the island,—distant four or five miles, in a north-easterly direction, from Gay Head, shafts have been recently sunk, thirty or forty feet, and white clay of a superior quality obtained. Large quantities are sent to Salem, Boston, New York, and other places, for the alum manufactories,—to be made into fire-proof bricks,—for the glass houses, and various other purposes. Formerly the Salem Chemical Laboratory was furnished with clay from Gay Head, and over six hundred tons were annually exported from thence, which cost, delivered on board the vessels, three dollars per ton. The beds are no longer wrought, and the Indians who were employed in collecting the purest kinds of white clay, are now at work in the shafts, near Mount Pleasant.

Clay appears in the north-western bank of Martha's Vineyard, at several places between Gay Head and Holme's Hole, and the superincumbent stratum is similar to that, which has been described, in the vicinity of the former place.

It is evident that the deposition of sand, gravel, and boulders, which rests upon the clay formation, was made by some great convulsion, or mighty flood, subsequently to that, which buried the primitive forests under its lofty strata, as there are none of the granitic boulders imbedded in them.

There are many thin strata of ferruginous stones, various ores of iron and arsenic, traversing those of clay, at Gay Head; and in one of them, about twenty feet below the surface, are imbedded many bones, apparently of whales, sharks, and other fish,—some of which are mineralized, but others still remain unchanged. Sharks' teeth of the size represented Pl. III, fig. 2, 3, are also met with.*

There are between two and three hundred Indians on Martha's Vineyard, who own about twenty-five hundred acres of land, including the peninsula, at the south-western end of the island, whose extremity is Gay Head. Their small and comfortless huts are not collected into villages, but scattered over the whole area of their sterile domain. They raise a little corn, some vegetables, keep a few cows and sheep, but depend chiefly upon fishing and digging white clay for a support. Some of the young men go "*a whaling*" from Nantucket and New Bedford, and make expert harpooners, and a few of them enter the merchant service.

From the best information I could obtain, the number is rapidly diminishing, from intemperance and poverty. The deaths among the children are much greater, than in the same number of white people.

Although they have continued upon the soil of their ancestors, and retained many of their native habits, they have no chiefs, or form of government; but what is most remarkable, they have entirely lost their aboriginal language, and speak only English. The period of their extinction can not be distant.

Upon visiting the light house on Bird Island, at the northern extremity of Buzzard's Bay, Mr Moore, the keeper, gave me specimens of a metallic substance, which has the appearance of iron ore, and is attracted by the magnet; still he observed, that with the usual tests there were no indications of that metal; and having paid considerable attention to chem-

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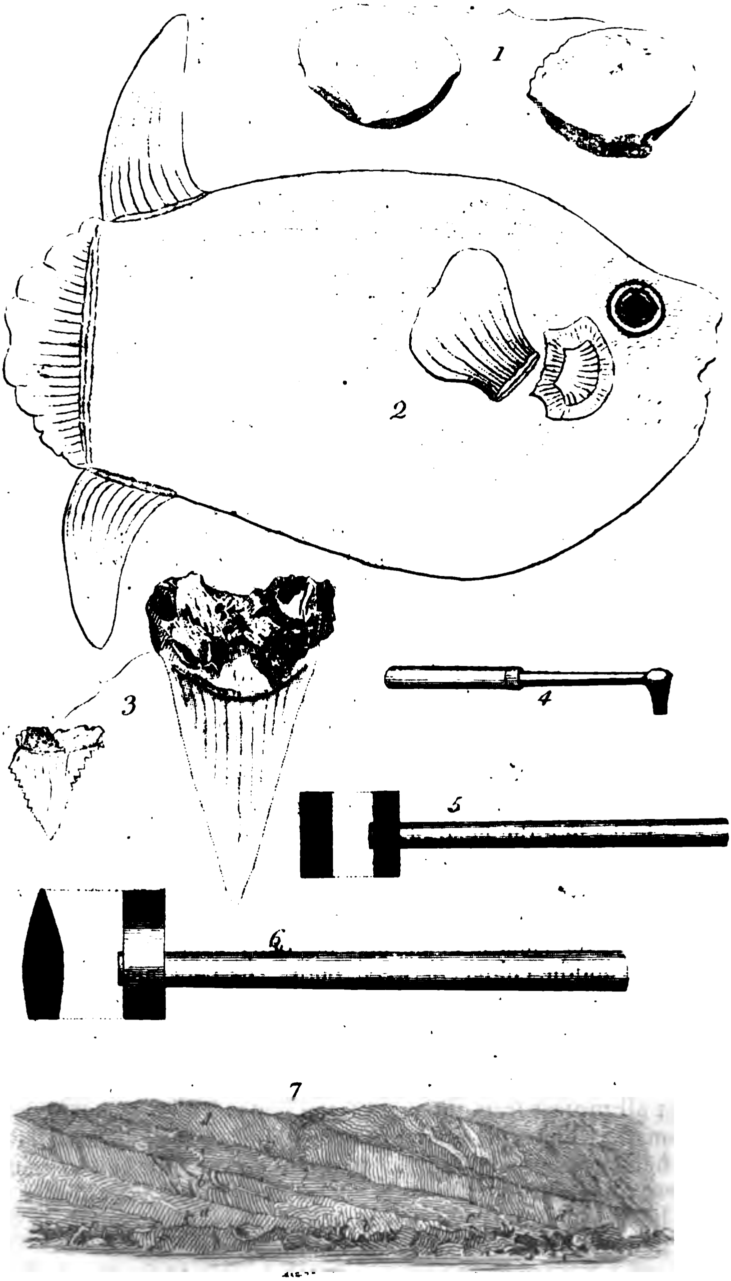
* And casts of a species of Venus. Pl. III. fig. 1. W.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istry and mineralogy, he thought it possible, that it might be columbium.*

Mr Moore first noticed this ore among the sand, on the beach, and afterwards detected it in several of the granitic masses of rocks which are scattered over the shores of the island, and extend a considerable distance into the bay.

On my return from the Vineyard Sound, being becalmed, off Chatham, an uncommon fish was discovered, near the cutter; I immediately went in pursuit in the boat, and succeeded in harpooning it. One of the lieutenants and a seaman, who had made several whaling voyages in the Pacific Ocean, recognized it, as a Sun Fish,—the Tetraoden Mola of Linnæus.

I made the accompanying sketch of it, (Pl. III, fig. 2). Length, 4 feet 1 inch. Width, 2 feet 2 inches. Length of dorsal fin, 20½ inches. Length of anal fin, 19 inches. Width of dorsal and anal fins, 9 inches. Length of pectoral fins, 6½ inches. Breadth of pectoral fins, 7 inches. Estimated weight, 200 lbs.

The following description by Willoughby, in his His. Pisc. of this remarkable fish, is generally correct.

" Sun-Fish,—Mola,—the Tetraoden Mola of Linnæus, and a species of ostracian, in the Artedian system in Ichthyology; a fish of a very singular figure.

" Its body is broad and short, and its hinder extremity is terminated by a singular fin which serves it for a tail; so that it looks like the head of a large fish severed from its body; it is frequently of two feet in length, and sometimes very much exceeds that size, growing to even two hundred weight. It has no scales, but is covered with a hard, harsh, and rough skin. Its back is black and its belly white; the sides, are of a middle colour between both. Its back and belly both terminate in a narrow edge. Its mouth is very small for the size of the fish, and when open is round. Its jaws are hard and edged like a knife, within; externally they are rough, as if beset with several rows of small teeth. The head does not at all project from the rest of the body. The eyes are very small. The gills are only two elliptic holes, covered with their proper membranes. Its flesh is very soft, and its bones are all gristly and tender. The skin sticks very firmly to the flesh, and is not easily taken off. It is caught in the Mediterranean and sometimes in the British seas."

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* On examination of the specimens, they proved to be titaniferous oxide of Iron. W.

The eyes, instead of being "very small," are as large as those of an ox, and being furnished with a nictitant membrane, when they are touched they are immediately withdrawn below the exterior surface, and enveloped by the folds of this membrane, which is thick and wrinkled.

The caudal fin is scalloped and has a motion just below the dorsal and pectoral fins, where the skin is wrinkled, as is the case at the base of the other fins.

This fish is never seen, but in calm weather, and more generally when the sun shines bright; it then rises near the surface, and the dorsal fin appears out of water. In swimming, it has a rocking motion, and occasionally turns on its side and gently floats along.

They are not common on our coast, but are plenty in the Pacific Ocean, and some have been caught which weighed 800 pounds. Parts of them are eaten by the whalemen, but not when other fish can be obtained. The gristly substance which constitutes a large portion of the fish, is very elastic, and when cut into balls they rebound like those made of caoutchouc.

Brinley Place, Roxbury, Aug. 28, 1826.

ART. LV.—*Notice of the Discovery and Geological Situation of the Native Gold of Vermont.*

In reply to a request for information in regard to the mass of native gold, lately found at New Fane in Vermont, we have been politely favoured with the following letter from General Field. Accompanying the letter was a specimen of the gold, which has been placed in the cabinet of Harvard College.*

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* In former numbers of this journal, particular notice was taken of all the additions to the cabinet of minerals at Cambridge, but as the publication of a list of minerals and localities could be interesting to but a small portion of our readers, it was discontinued Since that time the additions have been very numerous, so much so, indeed, that the spacious apartment in which the mineralogical collection is arranged, is insufficient for their advantageous display. Many donations have been made by mineralogists, and others, in all parts of the country, and several boxes have been transmitted by our countrymen resident abroad. Ample supplies of the minerals of New England have been obtained by occasional excursions, of which mineralogists are invited to avail themselves in exchange. Boxes may be addressed to the care of Hilliard, Gray, & Co., Boston.

A box of highly interesting specimens of the ores of South America has

Dear Sir,

Agreeably to your request, I have examined into the circumstances relating to the native gold, lately found in New Fane, Vermont, and the following statement is the result of my inquiries, which may be proved by indubitable testimony. The gold was found in the highway near the dwelling house of Samuel Ingram, by a son of his, and was, unquestionably, there deposited with a quantity of clay, sand, and gravel, which had, a few days previously, been thrown into the road, by the highway surveyor, in repairing a bridge, which had been injured by a freshet. Upon examining the excavation, made by the surveyor, from which the gold must have been taken, we find the soil to be alluvial, and it appears to have been brought down and deposited by a small rivulet, which descends from a high hill. It consists of thin strata of clay, sand, gravel, and water-worn stones. The clay contains a great quantity of yellow mica, which, from its brilliant lustre, is very deceptive, and, unless critically examined, would be considered pure gold.

The specimen of gold, when found, was invested with an incrustation of a brownish colour, which was readily removed by washing. Its weight was 8½ ounces, its form was conical, and there were firmly adhering to its base, a number of small, transparent rock crystals. The gold is soft, ductile, flexible, and malleable; its specific gravity is 16.5, and it has the metallic lustre of virgin gold. From the various tests, which have been applied to it, there can be no doubt as to the identity and purity of the mineral.

The soil in the region in which the gold was found, is argillaceous, and affords beds of excellent potter's clay. The rocks, *in situ*, are all of the primitive class, consisting of hornblende, hornblende slate, and green-stone porphyry, which

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been received from William Tudor, Esq., U. S. Consul at Lima; and several specimens from the late J. B. Dabney, Esq., Consul at Fayal, (Azores). To Dr Bouè we are indebted for a very complete and instructive suite of the formations of the south of France and Germany; to Professors Monticelli, Ferrara, Gismondi, and Carpi, for the minerals of Italy and Sicily; to Baron Lederer for some from Austria, and to M. M. Andrè, Brongniart, Hausmann Brooke, Meade, Heuland, Traill and others, for specimens from Great Britain and the continent of Europe.

A specimen of the gold of North Carolina was presented some time since by the Hon. Daniel Webster; several varieties of iron ores and other substances, by Israel Thorndike, Jr, Esq., Mr Quinby, and Mr Alger. Many interesting specimens have been received from His Excellency the Governor of Maine, General Dearborn, Professors Silliman, Hall, and Torrey, Messrs. Delafield, Allen, Fowler, Jackson, Emmons, Hammatt, Chapin, and others whose liberal contributions we have before had occasion to notice. W.

are often found alternating with mica slate. The sulphates of iron and alumina are abundant in the mica slate of that region. They are found efflorescing upon the surface of the rocks, and in dry seasons, large quantities of those salts might be collected, with but little labour.

I am, Sir, yours, &c.
MARTIN FIELD.

New Fane, Sept. 30, 1826.

P. S. With this, I send you a specimen of the gold above mentioned, for your examination. It was taken from the outer surface of the lump, and is as fair a specimen as I could procure.

ART. LVI.—*Notice of the Mineralogy of Nova Scotia, and of several New Localities of American Minerals.* J. W. W.

THE peninsula of Nova Scotia has long been known to contain large deposits of iron ores and coal, but has not, to our knowledge, been examined by any geologist. Of its mineralogy, we have been too long kept in ignorance, and although arrangements were made by the government for a mineralogical survey, we have not as yet met with any account of the progress of the work.

Some months since, being presented with specimens of several of the ores of iron from different parts of the peninsula, by Mr J. B. QUINBY, whose object in visiting the place had been to discover iron ores, much interesting information was obtained from him in regard to the localities. The ores are chiefly the magnetic, specular, and argillaceous. Some of them are found along the margin of St Mary's Bay, in horizontal strata of from 1 to 12 inches in thickness, separated by layers of greenstone. Others are met with in detached masses or boulders, and are sometimes nodular. These are more or less associated with granite boulders and masses of felspar and quartz.

An important locality of magnetic iron is at Clements. The rock which accompanies it is clay slate, and according to Mr Quinby the two pass into each other. It constitutes a "vein or bed" 10 feet wide, but as yet its extent cannot be well ascertained. Boulders of this ore are abundant over a district of country for 2 miles in length and from ¼ to ¾ of a mile in width, they are uniformly slaty in their structure. The course of the bed or vein is about N. E. and S. W. It appears to be solid and free from seams; it will probably require to be drilled and blasted, which will materially increase the expense of working it.

At Nictaurc the deposite of iron is of great extent, forming a bed extending several miles in a N. E. and S. W. direction. This ore is slightly magnetic, and constitutes a regular vein, which is 6 feet wide, the walls of the vein inclining outwards. The ore throughout is intersected by numerous seams running in every direction. This circumstance will greatly facilitate any mining operations, and admit the use of the wedge, by which masses of great size may be removed. It exhibits impressions of Terebratulæ, as does the adjoining slate, especially where in contact with or passing into the ore.

In one or two specimens portions of the original shell were found, but instances of this are rarely to be observed. Some of the seams are filled up with a substance like reddle.

The Moose river and Nictaurc beds of ore are distant from each other about 40 miles, and on the same range of hills which passes in a N. E. and S. W. direction through the county of Annapolis, nearly parallel with and about 15 miles S. of the Bay of Fundy. Clay slate is abundant in this range, and in many places is susceptible of being split into large slabs and tables. It was considered probable by Mr Quinby that these beds or veins of ore traverse the greater part of this range, from the magnetic needle being affected throughout the whole extent.

At Nictaurc the ore was formerly smelted at a small bloomery forge, but the iron is said to have been of inferior quality. This probably is to be attributed to the imperfect method of working the ore.

At Aylesford, argillaceous iron ore was found in several places, in great quantity, extending 6 or 8 miles along the course of Annapolis river.

Near the head of the Bay of Fundy, an ore was first obtained which was supposed to be manganese, and as such sent to the United States. It proved on examination to be magnetic iron with red oxide of iron, and a very small portion of oxide of manganese.

The specimens brought by Mr Quinby were lately followed by others still more interesting to the mineralogist, as fragments of amethyst, laumonite, and several analogous minerals. Arrangements made to visit these localities during the past summer were reluctantly abandoned in consequence of the long continued wet and unfavourable weather. Fortunately, however, Mr Francis Alger, who had been some months on the peninsula, returned with several boxes of different substances, which he had met with while engaged in searching for ores of iron. The examination of these substances has led to the knowledge of a new locality of many highly interesting and beautiful minerals, and Mr Alger has, with great liberality, allowed us to select from his collection a series of specimens, with permission to offer some account of them.*

The rock crystals which were obtained are of great size, one of these measures nine inches across each lateral plane of the prism, and the length of one of the accuminating planes is 10 inches. The weight of one of the crystals brought by Mr Alger is 90 pounds, the diameter of its base is 13 inches, and its length 19 inches. Both this and the smaller crystals and fragments are smoky, and yet quite transparent; in some the intensity of colour is nearly equal to that of the rock crystals found in several places in Scotland, and known in commerce as Cairngorums—the name being derived from the range of mountains in which they were first met with, and which has been retained by the lapidaries and applied to smoky and topaz coloured quartz from any locality. The Scotch rock crystals are obtained in the water courses and ravines in many parts of the Highlands, and the traveller often meets with small parties of children and individuals who are searching for them; they are mostly found more or less rounded by attrition, and it is somewhat rare to meet with a perfect crystal. The stones are sold to the lapidaries in Edinburgh and other large towns, by whom they are sometimes submitted to certain processes, by which the colour is heightened, diminished, or wholly removed. This is done by enclosing the mineral in charcoal, and submitting it to heat; or by heating it and plunging it into different coloured solutions. The largest specimen which has been found in Scotland, weighs about 78 lbs.

The beauty of the Nova Scotia rock crystals is increased by the long and slender prisms of black schorl which they contain; some of them are 3 inches in length and but $\frac{1}{16}$ in thickness.†

The specimens found by Mr Alger were loose and in the soil, but it is very obvious from the appearance of the exterior, that they were derived from the neighbouring ridges or veins of granite, as pieces of the other constituents of granite still adhere to them.

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* These have been placed in the cabinet of Harvard College.

Mr A. has had a seal of much beauty cut from one of these crystals.

Alternations of red and white sandstone, with veins of reddle, occur at Digby Neck, forming precipices one hundred or more feet in height. This deposite of sandstone deserves careful examination.

Amethyst; many rich and high coloured specimens of this mineral were obtained by Mr Alger. The crystals are associated with magnetic iron, and in some specimens with calcedony and jasper; most of the specimens have been obviously taken from geodes, but some of them are in thin and flat masses, having formed incrustations upon the ore. The principal locality is in the North mountains at Digby Neck. The ore itself, at this place, occurs in detached masses only, varying in weight from 1 to 10 cwt.

Specular iron ore; some of this is equal in beauty to that from Elba; it occurs at the head of St Mary's Bay.

By this interesting collection a new locality of laumonite is made known to us, and from the appearance of the specimens, this substance occurs in large masses and abundantly. It has in general a deeper red colour externally than the foreign specimens, and within is of a flesh colour more or less intense, and white. The principal locality is at Sandy Cove, in Digby Neck, where it forms numerous veins traversing a precipice of trap 60 or 70 feet in height, extending along the shore, and forming a barrier to the sea for a long distance. The veins are of various thickness and length, and nearly vertical. The laumonite is associated with exceedingly beautiful crystals of rhomb spar, chabasie, mesotype, heulandite, and ichthyophthalmite. These minerals are accompanied by small plates of specular iron ore, which from its brilliancy, has, till recently, been deemed an ore of silver, and been sought with avidity by persons from all parts of the peninsula.

Black oxide of manganese; in some specimens this substance is well crystallized; and was met with at the head of the Bay of Fundy. Radiated manganese occurs in loose masses at Lawrencetown about 20 miles east from Halifax.

Large masses, exceeding a ton in weight, composed principally of diallage, occur at Nictaure, and the same mineral is associated with iron pyrites at Clements.

Among the specimens were several others indicating the existence of trap rocks, as wacké, clinkstone, jasper, and distinct trap. There were a few pieces of sulphuret of antimony of great beauty, and of native copper from Cape Dory.

Sulphuret of iron occurs in considerable quantities in clay slate, near the mouth of Bear river. It is principally the amorphous variety, and by the action of air and moisture, effloresces, and is converted into sulphate of iron; it has never been worked, but will probably prove an abundant source of copperas.

Brown and red hematite, has been found in small quantity, in Digby, but not *in situ*; it is thought however, by Mr Alger, that an extensive deposite of these ores exists in the vicinity.

The minerals thus briefly noticed show that we have in our vicinity formations analogous to those of Iceland and Ferroe, and that this whole district deserves the careful examination of geologists.

New Locality of Scapolite, Rose Quartz, &c.—In the town of Freeport in Maine, within a few miles of Brunswick, I discovered a bed of limestone containing scapolite and pargasite. The portions of limestone upon the surface were full of these substances, and the crystals although much changed and injured from exposure, were quite distinct. The whole appearance of the limestone and its accompanying minerals is remarkably like that at Bolton in this State, and it is probable that beneath the surface the former will be found to contain as great a variety of substances. It is a primitive limestone, and in the immediate vicinity of large grained granite, but from the state of the field in which it was noticed, I was unable to ascertain its limits, or whether the two rocks were in actual contact.

The granite is in some places composed of gigantic crystals of felspar, and the quartz is rose coloured.

New Locality of Garnet, (Cinnamon Stone?) Sahlite, &c.—The limestone of Bolton appears to extend in a N. E. direction as far as Chelmsford, and in a late examination of this formation, I discovered at Carlisle, near Chelmsford, uncommonly fine crystals of dodecaedral garnet, idocrase (Egerane), sahlite, pargasite, and scapolite. The crystals are imbedded in the limestone, and when this is removed by the cautious use of acids, the garnets exhibit a most brilliant lustre, and perfect dodecaedral form; the angles and edges being often replaced. These crystals vary in size, some are quite small, while others are an *inch or more* in diameter. They are perfect gems, and will probably prove to be cinnamon stone, or essonite. It will depend, as Haidinger remarks, upon the investigations of the regular forms of cinnamon stone, whether or not it is to be considered as a species of its own. The close agreement of all its properties with those of dodecaedral garnet, and the tessular forms inferred from the observations of Biot, Brewster, &c. render it extremely probable that it is a variety of dodecaedral garnet. Robell, in a memoir on Garnet, maintains that cinnamon stone is a variety of garnet; and is disposed to consider helvin and the hyacinth red and orange coloured garnets of Piedmont as varieties of cinnamon stone.*

New Locality of Chabasie.—I have lately discovered crystals of chabasie in the sienite of Charlestown (Mass.) associated with prehnite.

Beryls of large size.—At Georgetown, Parker's island, at the mouth of the Kennebec river I found in a high ridge of granite, crystals of Beryl, exceeding fifteen inches in length, and six in thickness; one of these, now in the college cabinet, is acuminated, and of a fine blue colour and nearly transparent. They are associated with schorl of nearly equal size and in great abundance; and I have recently received from A. Hammatt Esq. of Bath, garnets and egerane, resembling those noticed above, discovered by that gentleman at Phippsburg on the Kennebec.

General Intelligence.

New Work on Mineralogy.—Dr J. L. Comstock, of Hartford, Conn. is publishing a Manual of Mineralogy, which, from a portion we have had the pleasure of examining, promises to be a valuable addition to the works on this science, and a convenient text book for students.

The Moon and its Inhabitants.—Olbers considers it as very probable that the moon is inhabited by rational creatures, and that its surface is more or less covered with a vegetation not very dissimilar to that of our own earth. Gruithuisen maintains, that he has discovered, by means of his telescope, great artificial works in the moon, erected by the Lunarians; and very lately, another observer maintains, from actual observation, that great edifices do exist in the moon. Nøggerath, the geologist, does not deny the accuracy of the descriptions published by Gruithuisen, but maintains that all these appearances are owing to vast whin-dikes or trap veins rising above the general lunar surface. Gruithuisen, in a conversation with the great astronomer Gauss, after describing the regular figures he had discovered in the moon, spoke of the possibility of a correspondence with the inhabitants of the moon. He brought, he says, to Gauss's recollection, the idea he had communicated many years ago to Zimmerman. Gauss answered, that the plan of erecting a geometrical figure on the plains of Siberia corresponded with his opinion, because, according to his view, a correspondence with the inhabitants of the moon could only be begun by means of such mathematical contemplations and ideas, which we and they must have in common. The vast circular hollows in the moon have been by some considered as evidences of volcanic action, but they differ so much in form and structure from volcanic craters, that many are now of opinion, and with reason, that they are vast circular valleys.—*Edin. N. Phil. Journ.*

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* See Edin. N. Jour. 395.

Morichini on Magnetism.—It results from the experiments of this distinguished philosopher, that there very probably exists a magnetic power in light, particularly in the exterior edge of the violet ray; and also, that this power is to be ascribed more to the chemical or deoxydising rays, than to the violet ray itself. If this newly discovered property of light shall be confirmed by the experiments of others, we must not, as some are disposed to do, abandon the idea of the earth's magnetism. The earth, as Morichini remarks, will absorb the magnetic fluid of the solar rays, as it absorbs heat and light. Iron will bear the same relation to the magnetic fluid, as pyrophorus to caloric and phosphorus, by isolation, to light.—*Ibid.*

Chloride of Lime as an Antiseptic.—The chloride of lime is remarkable for its antiseptic powers: thus, if an animal body already offensive owing to putrefaction, is drenched in an aqueous solution of this salt, the smell entirely disappears; further, if fresh flesh is drenched in it soon, that is in a few days, it becomes converted into a mummy-like whitish substance, and does not give out any unpleasant smell; hence it has been recommended to use chloride of lime, in preference to all other substances, in the embalming of bodies. ½ lb. of chloride may be added to 20 lbs. of water.

Structure of the Swiss Alps.—From the foot of the Cevennes.

by Marseilles, Gap, Grenoble, Geneva and Bex, similar and very simple geognostical relations occur. The lowest rock is blackish marly limestone, which, from its fossils, and other characters, appears to be a lias limestone; to this succeeds a white, compact, often oolitic limestone, which is Jura limestone, and is often covered with clay, sandstone, marl, &c. that belong to the quader sandstone and green sandstone. The valleys are often filled with Molasse. The Swiss Alps are, in all probability, similarly constructed. The dark transition limestone, with its subordinate beds of gypsum, belong to the lias formation; the true alpine limestone to the Jura limestone; the green sand and quader sandstone form the highest ridges of the calcareous alps, as on the Mount Saleve, Diablerets, &c. It seems problematical if true transition limestone occurs in this part of the Alps. If the view now given be correct, says Kefferstein, the calcareous alps and the Jura exhibit the same geognostical structure and composition, and probably were at one time connected together (as is the case at present in the south of France), forming an extensive plateau, which, at a period not very remote, suffered violent elevations and depressions, by which the green sand, for example, was raised to the height of 10,000 or 12,000 feet. These changes, which have given the present form to the Alps, may have taken place during or after the deposition of the chalk formation.—*Ibid.*

Strickland's Reports, &c.—This work, as most of our readers probably know, was published under the auspices of the "Pennsylvania Society for the promotion of Internal Improvements," at whose expense Mr Strickland travelled abroad to collect information, on the numerous subjects of Canals—Docks—Breakwaters—Turnpike Roads—Railways and Locomotive Engines—Horse-Gins and Cranes—Coke—the Manufacture of Iron—of Blistered and Cast Steel—Rollers for Bar and Hoop Iron and for Calico Printing—and Gas Lighting. The Reports contain a summary account of the public works and manufactures, as above enumerated, visited by him, while in Europe. Much the most valuable part of the work is the plates. These are about sixty in number, and are fair and clean impressions. In fact the written reports are little more than introductions and explanations of the plates. We have examined the whole with some attention, and do not hesitate to pronounce it a valuable work. Those who are yet to commence their researches in engineering and the arts, will find in it much information expressed in a clear and unequivocal manner; while the *initiated* will often refer to it for facts which have escaped their memory. Higher praise than this we cannot bestow upon it, for we are not aware that it contains any important knowledge, of which the public were not already in possession, in one form or another.

It is not the fault of Mr Strickland that he has not made any considerable addition to our information on the subjects of his reports; for the reports themselves furnish sufficient evidence that he was well qualified to have done so, if the society, whose agent he was, had permitted him to confine his attention to fewer objects. But it is beyond the reach of any one mortal intelligence, to become thoroughly acquainted with one quarter of the subjects that he was ordered to investigate, in the period of his travels.

We have no doubt but Mr Strickland will agree with us in this, indeed he has already recorded the same opinion, in his work; he says, in his report on the manufacture of iron, "I have not been able to find any one iron master acquainted with the whole ground, there is so much of the subject, connected with the manufacture of iron, depending upon tact, time, and practice, that it is almost impossible to reduce it to an exact system, that shall be perfectly understood by a single individual." Could it then be expected that Mr Strickland should possess himself, not only of the iron manufacture, but of a dozen other equally difficult subjects, in half as many months? Collect and make drawings of their architecture and machinery, and explain the whole to the public?

However it may be thought that the "Pennsylvania Society has fallen short of its object, by attempting too much; yet the effort itself deserves all praise, and we hope that the example of missions of this kind, will find so many imitators, that the attention of each may be narrowed to a single kind of public work or manufacture.*

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[*] At p 20, the following paragraph occurs. "The most approved ascents or angles of elevation and depression, on the English roads, vary from one inch in a yard, to one inch in a foot; the latter being considered the best." Probably the word *foot*, is a mis-print for *rod*. The error, whatever it is, will deserve correction in another edition, for which purpose we have noticed it.

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